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SEMI-MONTHLY.

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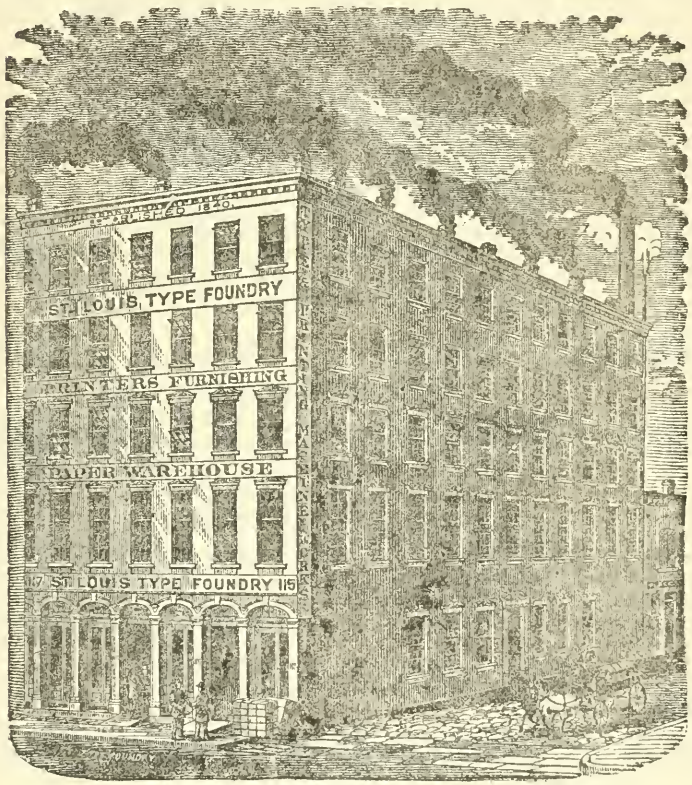

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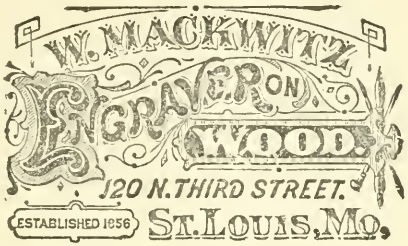

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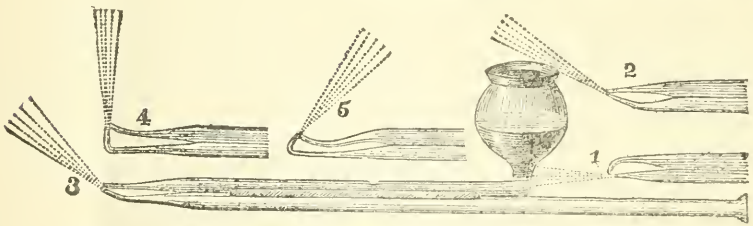

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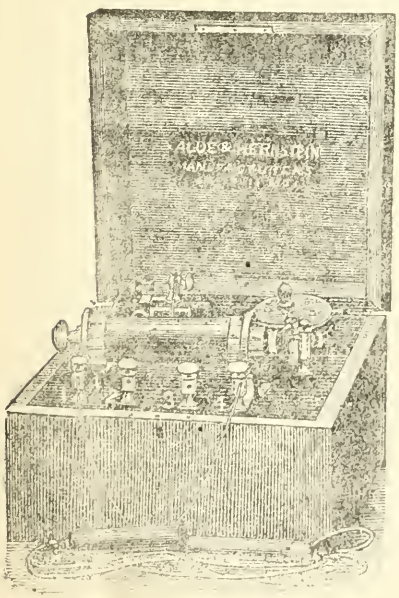

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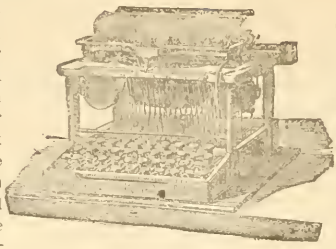

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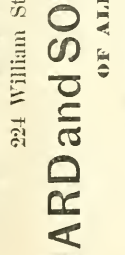

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Original Contributions.

ARTICLE XVII.

THE IDENTITY OR NON-IDENTITY OF MEMBRANOUS CROUP AND DIPHTHERIA. By H. Z. GILL, A. M., M. D., of Jerseyville, Ill. Committee on Croup reported to the Illinois State Medical Society, at Belleville, May, 1880. [Continued from page 137 of the JOURNAL.]

The two following tables from the London Committee are inserted, to show at a glance the relative value of symptoms in different forms of the membranous disease:

CLASS I.

FALSE MEMBRANE IN LARYNX AND FAUCES.

SEX:—

Males, 18. Females, 23. Not Stated, 1. Total, 42 cases.

AGE:—

From	1–2	2–3	3–4	4–5	5–6	6	
Males.	1	7	5	1	2	2	0=18
Females.	3	7	5	4	3	0	1=23
Not Stated.	1						1
Total.....							42

TIME OF YEAR.—Month of admission to hospital:

Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
8	2	5	4	0	6	4	6	3	3	4	2=42

ETIOLOGY :—

Other cases of diphtheria in neighborhood	7	12
Contracted in hospital	5	
Supposed due to catching cold	3	
No data	27	

EARLIEST LOCAL SYMPTOMS OF DIPHTHERIA :—

Sore throat	14
Croupy cough, hoarseness or dyspnœa	23
Sore throat and laryngeal symptoms simultaneously	2
Coryza	2
No date	2

TIME OF ONSET OF LARYNGEAL SYMPTOMS :—

Day of disease.	1	2	3	4	5	6	7	No date.
Cases.	16	4	4	3	3	1	1	5

TRACHEOTOMY :—

23 times and 1 recovery.

ALBUMINURIA :

Old nephritis before croup	2
Present during life	20
Urine not obtained during life. Albuminuria after death	2
Not albuminous so often as examined	3
No data	15

RESULTS :—

Died....18	Males.	22 Fem.	1 Unknown sex.....	41 Deaths.
Recov.. 0		1	0	1 Recov.

CLASS II.

FALSE MEMBRANE IN LARYNX ; NOT IN FAUCES.

SEX :—

Males 10. Females 11. Total 21.

AGE :—

From	1–2	2–3	3–4	4–5	5–6	6.7	11	
Males	1	3	3	0	2	0	1	= 10.
Females	1	0	1	4	3	1.1	0	= 11.
Total								21

TIME OF YEAR :—Admission.

Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1	1	3	4	2	1	0	0	3	2	4	0–21

ETIOLOGY :—

Attributed to exposure to cold	2
Contracted in hospital	4
Diphtheria in neighborhood (none said to be so)	0
Second attack of croup	2
During measles	1
During whooping cough	1
During scarlatinal dropsy	2
During typhoid fever	1
Scarlet fever in house	2
No data	8

EARLIEST LOCAL SYMPTOMS OF DISEASE :—

Sore throat	1
Laryngeal	17
Both at same time	1
Coryza	1
No data	1

TIME OF ONSET OF LARYNGEAL SYMPTOMS :—

Day of disease	1	4	6	Doubtful
Cases	15	2	2	2

TRACHEOTOMY :—

11 times and 2 recoveries.

ALBUMINURIA :—

Old nephritis before croup	2
Present during life	7
Not present so often as examined	6
No data	6

RESULT :—

Died	19	
Recovered	2	Both females.

The conclusions of the London Committee are so significant, and in many points so strongly in contrast with the opinions held by many of the profession that we deem it proper to insert them here:

"1. Membranous inflammation confined to or chiefly affecting the larynx and trachea may arise from a variety of causes, as follows:

"A. From the diphtheritic contagion.

"B. By means of foul water, foul air, or other agents, such as are commonly concerned in the generation or transmission of zymotic disease (though whether as mere carriers of contagion cannot be determined).

"C. As an accompaniment of measles, scarlatina, or typhoid, being associated with these diseases independently of any ascertainable exposure to the special diphtheritic infection.

"D. It is stated on apparently conclusive evidence, although the committee has not had an opportunity in any instance of examining the membrane in question, that the membranous inflammation of the larynx and trachea may be produced by various accidental causes of irritation, the inhalation of hot water or steam, the contact of acids, the presence of a foreign body in the larynx, and a cut throat.

"2. There is evidence in cases which have fallen under the observation of members of the committee and are mentioned in the tables appended, that membranous affection of the larynx and trachea has shortly followed exposure to cold, but their knowledge of the individual cases is not sufficient to exclude the possible intervention or coexistence of other causes. The majority of cases of croupal symptoms definitely traceable to cold, appear to be of the nature of laryngeal catarrh.

"3. Membranous inflammation, chiefly of the larynx and trachea, to which the term 'membranous croup' would commonly be applied, may be imparted by an influence, epidemic or of either sort, which in other persons has produced pharyngeal diphtheria.

"4. And conversely, a person suffering with the membranous affection chiefly of the air-passages, such as would commonly be termed membranous croup, may communicate to another a membranous condition limited to the pharynx and tonsils, which will be commonly regarded as diphtheritic."

The farther remarks are added:

"It is thus seen that the membranous affection of the larynx may arise in connection with common inflammation or with specific disorders of several kinds, the most common of which in this relation is that which produces similar change elsewhere, and is recognized as diphtheria.

"In the large number of cases of membranous affection of the larynx the cause is obscure (i. e. in any given case it is difficult to predict the particular cause in the case).

"Among those in which it is apparent, common irritation seldom presents itself as the source of the disease, accidental injury is but very infrequently productive of it. But few cases of undoubted origin from exposure to cold are on record. On the

other hand, in a very large number of cases infective or zymotic influence is to be traced.

"The membrane, even when chiefly laryngeal, is more often than not associated with some extent of a similar change in the pharynx or on the tonsils; and whether we have regard to the construction of the membrane, or to the constitutional state, as evinced by the presence of albumen in the urine, it is not practicable to show an absolute line of demarcation (save what depends upon the position of the membrane), between the pharyngeal and laryngeal forms of the disease.

"The facts before the committee only warrant them in the view that when it obviously occurs from a zymotic cause or distinct infection and primarily affects the pharynx, constitutional depression is more marked, and albuminuria more often and more largely present, though in both conditions, some albumen in the urine is more frequently present than absent.

"The most marked division indicated by the facts before the committee is that between membranous and non-membranous laryngitis.

"The committee suggest that the term *croup* be henceforth used wholly as a clinical definition implying laryngeal obstruction occurring with febrile symptoms in children. Thus croup may be membranous or not membranous, due to diphtheria or not so.

"The term diphtheria is the anatomical definition of a zymotic disease which may or may not be attended with croup.

"The committee propose that the term membranous laryngitis should be employed in order to the avoidance of confusion whenever the knowledge of the cases is such as to allow of its application."

CONCLUSIONS.

From our observations and examination of the subject the following conclusions are arrived at:

1. There is no distinction to be made from a microscopic examination of the exudate which would lead to a division of membranous laryngitis into non-diphtheritic and diphtheritic, or simple membranous croup and diphtheritic croup.

2. There is no distinction clear and decisive to be made based on chemical examination of the membranous product.

3. Every clinical manifestation by which a distinction might at first appear between diphtheritic and non-diphtheritic mem-

branous laryngitis is unreliable in a differentially diagnostic point of view.

There is evidence tending to show the action of some unknown influence, in some respects analogous to the so-called zymotic poisons, in the large majority of cases of membranous laryngitis.

The nature of this cause, whether identical with that of diphtheria, or only producing in the larynx, trachea and pharynx products indistinguishable from it by our present methods of investigation is not decided, though the weight of evidence would seem to indicate a close analogy, but possibly not an identity with it in some of the purely laryngeal cases of membranous formation, such as are commonly termed idiopathic croup.

We would submit the following scheme for collecting statistics on the subject in order to uniformity, and to obtain from various sources information of importance on the principal points:

1. Name.
2. Sex.
3. Age. { Years, / Months.
4. Cause, whether from exposure to cold, or diphtheritic contagion, etc.
5. Croup or Diphtheria.
6. Preceding symptoms and onset.
7. False membrane observed at any time or not.
8. Condition of urine as to albumen.
9. Treatment. { Medical, / Surgical.
10. Result.
11. Date of operation.
12. Name and address of operator.
13. Anæsthetic.
14. High or low operation.
15. Immediate cause of death.
16. Duration from onset to operation, and to recovery or death.
17. Tube worn, how long.
18. Were there cases of diphtheria in the neighborhood?

Remarks.

LIST OF CASES ON WHICH TRACHEOTOMY HAS BEEN PERFORMED FOR CROUP OR DIPHTHERIA—CONTINUED FROM 1879.

NO.	NAME.	AGE.	CAUSE.	RESULT.	RESIDENCE.	OPERATOR.	DATE.	REMARKS.
107	Henry Johnson	6 years.	Croup.	Recovered	Pittsfield, Ill.	Drs. Ledlie and Casal.	Jan. 8, 1880.	Had been sick thirty-six hours. Gave ether. Wore the tube five days. High operation. "Case was exceedingly favorable for the operation." "Had excellent after-treatment
108	Oliver Martin, Jr.	4 yrs. and 6 mos.	Diphtheria	Died.	Pike Co., Ill.	Drs. F. M. Casal and Albt. Reynolds.	Oct. 17, 1879.	Had been sick five days. No anæsthetic. Lived twenty-three hours. Immediate cause of death, asphyxia. Condition—in articulo mortis. High operation. Seven miles distant. "It is alleged that the boy died while the attendants were attempting to replace the tube, having removed outer tube to clean it."
109	Rosanna M. Nast, (female.)	1 yr. and 8 mos.	Croup?	Died.	Walkervill, Green Co., Ill.	Dr. G. W. Lasher.	Jan 10, 1880.	Previous illness about four days. High operation. Lived thirty hours. Gave ether. Removed false membrane 1 1-2 inches long, 1-4 inch wide, through opening. Complete aphonia, cyanosis and labored respiration.
110	Dolly Hasselton, (female).	4 years?	Membranous Croup.	Recovered	Carrolton, Green Co., Ill.	Dr. G. W Lasher.	Oct. 1879.	Had been sick four days. Gave ether. High operation, hemorrhage troublesome. Temperature from 103 to 104° for 48 hours. Resp. 40 to 45. Wore tube 6 days. Relief immediate.
111	(Female.)	9 years?	Diphtheria	Died.	235 Ogden Ave. Chicago.	Dr. D. W. Graham assisted by Dr. E. F. Ingals.	Mar. 1880.	Had been sick four or five days. Lived 36 to 48 hours. Died from constitutional results of diphtheria. Gave ether. High operation. Patient rational. Laborious respiration. A tube was worn till death

LIST OF CASES ON WHICH TRACHEOTOMY HAS BEEN PERFORMED FOR CROUP OR DIPHTHERIA—CONTINUED FROM 1879.

NO.	NAME.	AGE.	CAUSE.	RESULT.	RESIDENCE.	OPERATOR.	DATE.	REMARKS.
112	Miles (male.)	6 years.?	Diphtheritic Croup.	Recovered	Chicago, Ill.	Dr. E. F. Ingals.	Mar. 15, 1880.	Previous sickness not given. "Breathing laborious, and much exhausted, but not markedly cyanotic." Artificial respiration for twenty minutes after the operation was employed. Tube removed on the 12th day. "Operation comparatively early."
113	Freer. (?)	2 years.?	"True Croup."	Died.	Chicago, Ill.	Dr. E. F. Ingals.	April 6, 1880.	Unconscious and extremely cyanotic. No anæsthetic. Used silk thread in the margin of the tracheal wound to hold the wound open. No tube at first. One used after twelve hours. Lived thirty-nine hours. Previous symptoms for two or three days. Had previously been treated with emetics freely. Cause of death, obstruction below the trachea. Death quiet and without a struggle.
114	L. Gingsbury. (?)	6 or 7 ys.	"True Croup."	Died.	Chicago, Ill.	Dr. E. F. Ingals.	April, 1880.	Previously ill about two days. Cyanotic, restless, breathing laborious and nearly unconscious. Chloroform in small amount. Lived forty hours. Ligatures used in cut edges of the trachea. Death from obstruction below the trachea. Death quiet and without a struggle.
115	(Female.)	4 years?	Croup.	Died.	Chicago, Ill.	Dr. H. A. Johnson	In winter of 1879 & 1880.	Did well for first three days. Died on the 6th day, of broncho-pneumonia. Disease entirely laryngeal.

LIST OF CASES ON WHICH TRACHEOTOMY HAS BEEN PERFORMED FOR CROUP OR DIPHTHERIA—CONTINUED FROM 1879.

NO.	NAME.	AGE.	CAUSE.	RESULT.	RESIDENCE.	OPERATOR.	DATE.	REMARKS.
116	Elnzvall (male).	3 years	Membranous croup.	Recovered	West Indiana St. Chicago.	Dr. Christian Fenger, Surg. to Cook Co. Hosp.	Oct. 18, 1878.	Previous illness, three days. Anæsthetic none. Dyspnœa urgent; pulse and temperature high. Tube worn five days.
117	F. Gehrig (feml).	2 years	Diphtheria	Died.	Chicago.	Dr. Fenger.	Oct. 25, 1878.	Previous illness ten days. Died October 26. Immediate cause of death "collapse;" condition at the time of operation—collapsed; high operation.
118	H. Kenzie (male).	8 years	Diphtheria	Died.	Penn St. Chicago.	Dr. Fenger.	Sep. 30, 1879.	Previous illness eight days. Lived four hours after operation. Immediate cause of death, collapse, hemorrhage. Severe dyspnœa, extensive diphtheritic exudation. Marked symptoms of blood poisoning anæsthetic, chloroform.
119	Wm. Archer (ml).	3 years	Diphtheria	Died.	Chicago, Ill.	Dr. Fenger.	Oct. 27, 1879.	Previous illness ten days. Lived twenty hours. Immediate cause of death, exhaustion. Anæsthetic, none. Diphtheritic exudation on tonsils, soft palate and extending into larnyx. Pulse rapid and feeble, blood poisoning. High operation.
120	O. Malkon.	4 years	Diphtheria	Recovered	Chicago, Ill.	Dr. Fenger.	Dec 24, 1879.	Previous illness seven days. Tube worn 14 days. Anæsthetic, none. Tonsils and soft palate covered with diphtheritic exudation. Pulse and temperature high. High operation.
121	Lucas West (ml).	3 yrs. & 3 mos.	Diphtheria	Died.	South Dearborn St., Chicago.	Dr. E. W. Lee.	Aug. 16, 1879.	Lived sixty hours after operation. Immediate cause of death—Asphyxia from extension of membrane. Anæsthetic, chloroform. Impending suffocation at time of operation. Low operation.
122	Aaron West (ml).	5 years.	Diphtheria	Recovered	S. Dearborn St., Chicago.	Dr. Lee.	Aug. 17, 1879.	Duration of previous illness five days. Tube worn one week. Pulse, 160; Resp., 60; Temp., 103°. Low operation.
123	Norah Ryan (fm).	2 years.	Membranous croup.	Died.	18 Gurley St., Chicago.	Dr. Lee.	Dec. 27, 1879.	Duration of previous illness one week. Lived 14 hours after the operation. Immediate cause of death—exhaustion. Anæsthetic, chloroform. Pulse, 168; Resp., 56; T., 103½°. Low operation.

LIST OF CASES ON WHICH TRACHEOTOMY HAS BEEN PERFORMED FOR CROUP OR DIPHTHERIA—CONTINUED FROM 1879.

NO.	NAME.	AGE.	CAUSE.	RESULT.	RESIDENCE.	OPERATOR.	DATE,	REMARKS.
124	W. Madden (ml).	3 years.	Diphtheria	Died.	18th Street, Chicago.	Dr. Lee, assisted by Dr. Guerin.	Jan. 1, 1880.	Duration of previous illness three days. Lived 24 hours after operation. Immediate cause of death, Asphyxia. Anæsthetic, none. Pulseless, deeply cyanosed. Respiration almost ceased. Low operation rapidly performed.
125	Alice Green.	3 yrs. & 3 mos.	Diphtheria	Died.	Blue Island, Illinois.	Dr. Lee, assisted by Drs. Herman and Kauffman.	Jan. 19, 1880.	Previous illness eight days. Lived six days. Cause of death, pneumonia. Anæsthetic, chloroform. Pulse, 144; Resp., 48; Tempt., 103°. Low operation.
126	C. J. Byrne (ml).	3 1-2 yrs.	Diphtheria	Recovered	W. Randolph St., Chicago.	Dr. Lee, assisted by Drs. W. E. Clark and Norman Bridge. Son of Dr. Byrne.	Feb. 5. 1880.	Previous illness, seven days. Tube worn seven days. Anæsthetic, chloroform. Pulse 144; Resp., 64; Tempt., 101°. Exudation of membrane over soft palate and tonsils. Low operation.
127	H. Graft [male].	7 years.	Diphtheria	Recovered	166 Brown St., Chicago.	Dr. Lee, assisted by Dr. M. Lennem	April 20. 1880.	Previously ill ten days. Tube worn five days. Anæsthetic, chloroform. At time of operation, pulse, 132; Resp., 48; Temp., 102°. Head thrown back, lips blue, face pale; epigastric retrocession. Two hours after operation: Pulse, 112; Temp., 101°: Resp. 36. Breathing with perfect freedom. High operation.
128	Jos. Menely [ml]	8 years.	Diphtheria	Died.	Greenfield, Green Co., Ill.	Dr. W. C. Day, Greenfield.	Nov. 30. 1878.	Duration of previous illness four days. Died on table during operation. Cause of death, asphyxia from extensive false membrane. Anæsthetic, chloroform. High operation.
129	Wm. Henderson [male]	4 years.	Diphtheria	Died.	Greenfield, Green Co., Ill.	Dr. H. W. Chapman, White Hall, assisted by Dr. W. C. Day.	Dec. 7, 1879.	Previous illness six days. Tube worn 3 days. Lived 3 days. Immediate cause of death, asthenia from Diphtheria poisoning. Anæsthetic, chloroform. Condition at time of operation, very anæmic. High operation.

Total, 129. Recoveries, 36. Died, 93. Per cent of recoveries, 27.9. Died on the table or almost immediately, 9; of these two took ether, two chloroform; the anæsthetic not reported in four, and in one of the nine nothing was given=9.

Article XVIII.

The Method of Air-Supply to the Middle Ear; the Density of the Air in this Cavity; the Cause of the Uniform Concavity of the Membrana Tympani, and the Functions of the Eustachian Tube.* By Thos. F. Rumbold, M. D., of St. Louis. [Continued from Page 156.]

In this chapter I propose to give a few experiments on the healthy ear, that will give additional proof that the "deglutition method" is not founded on facts. I do this both to show the rich resources that I have to corroborate the correctness of my views, and because some have said that I have taken pathological Eustachian tubes, to show their normal action, a seemingly defective method of getting proof.[1] That the pathological condition of the tubes of those patients did *not* produce the phenomena described—aside from causing the patency—and that the patency alone *did* produce these phenomena, is proved by the fact that on the *instant* that the tubes were closed, even before the inflammation that caused the patency had subsided, the whole train of phenomena *as instantly* disappeared, remaining away while the tubes remained closed. The patients, inability to keep the tubes permanently closed caused them to seek the assistance of a physician, otherwise they would not have complained of their hearing. This effectually answers this adverse criticism.

EXPERIMETS.

It is universally accepted as true, that the act of deglutition, in immediately removing the prominent symptoms, and the main disability to the hearing, occasioned by the inflation of the

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*From advance sheets of "The Hygiene and Treatment of Catarrh" [now in press]; 12 mo. pp. 500. [Geo. O. Rumbold & Co.]

1. Since the publication of the first article on the Eustachian tube, I have received quite a number of communications from physicians, mostly from the East, all but one agreeing that I had proved the correctness of my Seven Conclusions. This one, rather unphilosophically, I think, stated that "it is not possible to prove the physiological function of an organ by its action while in an abnormal state." This criticism has been anticipated long ago.

middle ear, is proof that the Eustachian tube was opened into the cavity, and that the restoration of the *usual* degree of hearing is complete. This has been accepted unquestioned, but if the hearing had been tested by exact measurement during the experiment, a very great error would have been discovered.

The logic of the profession runs thus: "Yes, the act of deglutition, as Toynbee said, opens the Eustachian tube all the way into the middle ear, so that air can freely enter or recede, therefore, the air density in the cavity is equal with the surrounding atmosphere; this is known from the fact that when the ear is either inflated or air is abstracted from it—the effect of which is to cause a disagreeable sensation and a deafness—the act of swallowing relieves both symptoms."

I am satisfied that if this experiment had been thoroughly tested, the functions of the Eustachian tube would have been discovered long ago. This error not only led to the deception of observers, but caused them to make other errors, as for instance, considering the tube as a means for draining the middle ear.

The following method of making this experiment, with a normal ear, will prove the correctness of what I have said:

First Experiment.—First, attach one end of a long thread to a watch, retaining the other extremity in the hand, then find the utmost hearing distance from the watch (in the usual method for testing the hearing), placing the taut thread up to the nose; the length of this thread, from the nose to the watch, being the normal hearing distance from the watch. Second, now inflate the tympanic cavity by the Valsalvian method, then immediately perform the act of deglutition, and instantly note the hearing distance from the watch on the thread. *The distance will be found to be a little less than it was before the inflation,* only returning again to the usual hearing distance *after* the elapse of a short period of time.

The following are the reports of a few of my friends, who tried this experiment at my request:

"Feb. 9th, 1868.—I heard the watch nearly twelve feet. I then blowed my breath in my ears, producing a great fulness. After I swallowed some saliva, this left me entirely; but I had to step half a step nearer the watch to hear it, and in few minutes, not more than two or three, I heard it again better than before. "JAMES E."

"July 29th, 1872.—After repeated efforts, I overcame the desire to swallow a second time, and found that the usual hearing returned in from three to five minutes after the first act of swallowing. If I swallowed two times, it returned sooner; even if I swallowed as often as I desired, I was still compelled to wait certainly over half a minute for the usual hearing to return. "J. C. W., Lebanon, Ill."

"Aug. 6th, 1872.—(Watch covered). Inflated my ears, and immeditely swallowed twice, and heard at second knot (14¾ inches), natural, in 26 seconds [23 inches].
Rev. E. A. H., Collinsville, Ill."

This shortening of the hearing distance demonstrates that the air did not all escape from the cavity during the act of deglutition (1st and 2d Cons.). If the Eustachian tube was opened all the way into the middle ear, the air within and without the tympanum would have been instantly equalized, allowing the membrana tympani as instantly to resume its normal position for as perfect hearing as there was before the inflation. Evidently there was a small portion of the tube that was not opened by the action of the tensor and lavator palati muscles, and, that the part or portion of the tube that was not opened, offered so little resistance to the passage of the superabundant air contained in the cavity, that the tensor tympani muscle, assisted by the natural resilience of the membrana tympani, was able to force out a sufficient quantity of this air to relieve the ear of the prominent symptoms occasioned by its presence; still leaving too much air in there for the usual hearing to return, compelling the experimenter to wait a short time for the mucous membrane of the middle ear and mastoid cells to absorb the remaining or superabundant portion (that the act of deglutition could not give opportunity to escape), which allowed the membrana tympani to become more flat than normal. After waiting for the consummation of this rarefying process the usual degree of hearing returned.

I will now make a few extracts from recognized authorities, with the view of bringing prominently before the mind, that it is a requisite of normal hearing that there should be a uniformity of air density in the tympanic cavity; and by the same authorities, and by experiments, and cited cases, I will show that there is a continual absorption of air in the cavity, and, consequently, there must be a correspondingly uniform supply of air to the cavity (3d Con.).

In the quotations, I wish the following points to be kept prominently in view: First, the assumed necessity of uniformity of air density in the cavity for the normal hearing. Second, the necessity for a method of supply to maintain this uniformity; therefore, the problem presented by them for solution is this: how can these two imperative necessities be maintained at the same time? Toynbee, Tröltsch, Roosa and others affirming that this is done by the action of certain muscles, opening the Eustachian tube all the way into the cavity during the act of deglutition.

Toynbee, on page 192, in explaining his method of maintaining a uniform air density in the tympanum, says: "During the few moments that the faucial muscles are brought into play in the process of deglutition, air can either enter or recede from the tympanic cavity, and thus be always of the same density as the outer air." On page 193 he gives his reasons why a constant supply of air is necessary, saying: "Although from the preceding remarks there can remain little doubt that the faucial orifice of the Eustachian tube is ordinarily closed, except during the act of deglutition, it is requisite to perfect hearing that the tube should be pervious, and that there should be a constant interchange of air in the cavity of the tympanum. If the Eustachian tube becomes impervious, the air that was in the tympanum at the time of the closure gradually disappears. It is not easy to decide whether it is absorbed, or whether by a kind of exosmose it passes through the membrana tympani; but whatever the cause, in a space of time varying in different cases from a few hours to a day or two, there is no doubt the air in the tympanic cavity becomes partially exhausted. The effect is to produce an increased concavity in the external surface of the membrana tympani; a forcing inward of the chain of ossicles; pressure on the contents of the labyrinth; and a very serious diminution of the hearing power."

In the last edition of Tröltsch on the Ear, page 180, the author, speaking of the Eustachian tube, says: "It serves as an outlet for the secretion of the latter (the cavity), but especially as a passage for the renewal of the air in the middle ear. It is, therefore, a ventilation tube, by means of which the meeting of strata of air of equal density before and behind the drum is made possible, and the air in the tympanum maintained of the same degree of tension as that of the external atmosphere."

Again, on page 187: "But if the tube is at the same time to be a ventilation tube, by means of which a regular exchange of air between the pharynx and the cavity of the tympanum is brought about, it is necessary that its regular and frequent gaping or opening should take place only in this wise, that the strata of air before and behind the membrana tympani be kept of the same tension and density that is requisite for a normal vibra-

tory capacity for the drum. Experience has shown that such an opening of the tube takes place with every act of deglutition."

My effort will be to demonstrate that this mode of replenishing air to the middle ear is not so "frequent" as to admit of its being as regular or uniform as the abstraction of air from the cavity—that is to say, that the mucous membrane, in absorbing the air, is more uniform than is the act of deglutition.

It seems to me that it is superfluous to say, that there is no possibility of there being a *uniform* supply of air by an *irregular* action of an organ; yet, according to the quotations, the cavity must await upon action for its air. Keeping in mind the constancy and rapidity of air absorption within the cavity, the longer the interval of time between the acts of deglutition that supply the air, the greater must be the variation between the internal and external air densities; therefore, good hearing must of necessity be of an intermittent character, synchronous with the act of deglutition. The following experiment will prove that this is not the case:

Second Experiment.—Let the experimenter place a watch (with a thread attached to it and employed for the same purpose as mentioned in the First Experiment) as far from his ear as he can hear it, marking this place on the thread by a knot; let him remain in this position without performing the act of deglutition for a number of hours. It will be found that his hearing the tick of the watch at the end of two hours will be just as acute as at the beginning of the trial, and that he could hear the sound at any time he turned his attention to it during this experiment.

The following are the reports of a few of my friends who attemped this experiment. I wish to say here, that it is no easy task to sit for several hours under prohibition not to swallow, as the mere knowledge that one should not swallow, intensifies the desire to do so.

"Jan. 10th, 1868.—I commenced to listen to the tick of father's watch at 3 o'clock, but was compelled to swallow before the first three minutes passed, then I did not swallow for nearly twelve minutes, then I swallowed in five minutes, the next time I did not swallow for nearly one hour, but my throat (by this time) was very dry. I heard the watch one time as well as the other.

Jennie E. D."

"Feb. 10th, 1872.—Commenced to listen to the watch (covered) at 2 o'clock P. M., and swallowed only when I was compelled to, at 2:03, 2:06, 2:26, 3:03, 3:16, 4:08, 4:25½, 4:44 and 5:37. o'clock P. M. I heard the watch equally well all the time, but my throat was a little sore when I got done.

BELLA R."

"March 21st, 1872.—I made the same experiment, and swallowed at 1, 1:08, 1:46, 2:20, 4:13, 5:12, 6:27 o'clock P. M. Distance just six feet. I heard the watch this time also as well at one time as another. My throat was not affected in the least.

BELLA R."

"April 8th, 1872.—Commenced to listen at 9 A. M., and did not swallow, because my throat was very sore (occasioned by tonsillitis), until 11:30 A. M., and then only because I forgot. Commenced again at 1:30 P. M., and did not swallow for just three hours. My hearing was not very good when I started, but was equally good at any time I listened to the tick of the watch.

H. LEE J."

Jan. 1880.—I had no difficulty in making the experiment you desired, as my throat was so very sore that I would not have swallowed for fifty dollars. (He suffered from a severe attack of acute tonsillitis). After having my throat sprayed at 7 A. M., I swallowed some milk; I did not swallow again until at 3 P. M. of the same day. While I had severe pain in my left ear especially, my hearing in this ear was equally good before and after swallowing. My watch was heard at about 47 inches *all* the time. G.—

It is certain that in the intervals between the act of swallowing there could not have been as uniform a supply of air to the tympanum, if the act of deglutition was the only means of air supply, as there must have been a uniform absorption of air within the cavity, and yet the hearing is just as acute after this long non-performance of the act that Toynbee, Tröltsch and others allow for the renewal of air to the cavity, as before the experiment, when they swallowed as often as they desired.

The result of this experiment proves one of two things, viz: That either there was no rarefaction of air within these tympana by absorption, or that the supply of air to the cavities must have permeated the Eustachian tubes as continuously as it was requisite to maintain the normal tympanic air density. As air absorption by the cavity is a known fact, it follows that my third conclusion is correct.

Every case of that numerous class known as "Obstructed Eustachian Tube," caused by chronic inflammation of the

mucous membrane lining it, will bear the same evidence as this experiment and the first one—viz: that at the act of deglutition, the Eustachian tube is not opened into the tympanum, yet the degree of their hearing proves that the air slowly and continuously permeates through the abnormally closed tubes into the middle ear, although not sufficiently rapid for good hearing. That there is air permeation in this condition of the tube, in all such cases of this class, is evident, because of the degree of hearing being much greater, and the concavity of the membrana tympani much less, than in patients suffering acute tubal catarrh, where little or no air can enter, because of excess of secretion. If air does not enter the cavity at all, then the deafness would be as great as in any case of complete closure of the tube, and the concavity of the membrana tympani would also be as great, for the length of time to allow air absorption is certainly long enough to accomplish this. It is also just as evident that the air does not enter the tympanic cavity during deglutition, for if this act opened the tube, what would hinder the hearing from being instantly increased, as is done by the inflation of the cavity by the air douche? That the hearing is increased by this inflation, I take it, is strong evidence that the Eustachian tube was not opened into the cavity at the act of deglutition. It may be claimed that the action of the tensor and levator palati muscles in those cases do at least give some opportunity for air to enter the cavity, but not sufficient on account of the thickening of the mucous membrane of the tube. Then relief for all such cases is constantly at hand; is it not evident that the prescription to perform frequent acts of deglutition would be the remedy, thus allowing more air to enter the cavity; making the air douche a needless operation; also, that the continued swallowing would increase the hearing, until the one-sided pressure on the membrana tympani was neutralized, and its normal position attained?

The following experiment will not only corroborate the views here expressed, concerning air permeation through the normally closed Eustachian tube, but will show more demonstratively than any experiment yet given, that air can enter the tympanum without the aid of the act of deglutition, even more rapidly than is required; showing, that there is no necessity for any action of the tensor and levator palati muscles to open this passage to the middle ear for the normal renewal of air.

Third Experience.—First find the utmost hearing distance from the watch, with the aid of the thread, as before stated; note this. Second, close the nostrils with the thumb and forefinger, and perform the act of deglutition several times, thus abstracting the air from the middle ear, so as to decrease the hearing as much as possible; immediately after this again find the hearing distance from the watch, and note this on the thread; continue every five minutes to make observations of the hearing distance for a period of half an hour, all this time refraining from performing the act of deglutition. It will be found that the hearing distance from the watch will continuously increase, although there has been no act to open the Eustachian tube.

The following shows the result of this experiment made by two of my friends:

"COLLINSVILLE, ILL., April 24th, 1872.—Found the normal hearing (watch covered) at length of this (shorter) string (48 inches) at 9 A. M.; swallowed with nose closed and heard at first loop (38½ inches) at 9:04 at original distance. Swallow and heard at same. Repeated several times.

Rev. E. A H."

"ST. LOUIS, April 24th, 1872.—First found distance (64⅝ inches) that I heard the watch, then I cut the cord, closed my nose and swallowed, at 12:45 P. M.

At 12:45	P. M.,	and heard at	1st knot	(48¼	inches.)
At 12:50	"	"	"	2d	"	(60⅝	"
At 12:52	"	"	"	full length		(64¾	"
Second trial.—At 8:35	P. M.,	heard at	1st knot			(49⅜	"
At 8:40	"	"	2d	"	(54¾	"
At 8:45	"	"	3d	"	(61⅝	"
At 8:50	"	"	4th	"	(62⅞	"
At 8:65	"	"	5th	"	(67	"
At 8:57	" same as at the start					(69⅛	"

AMELIA F.
(Teacher in one of the Public Schools.)

Evidently, the cause of this increase of hearing distance from the watch, under these circumstances, is due to the tympanum being again replenished with air, so as to allow the membrana tympani to return to its normal position. But how did this air get into the cavity?—by what avenue? I take it that this is as conclusive evidence as could be required to prove air permeation through the normally closed Eustachian tube (3d Con.).

Although the celerity of the air exhaustion within the cavity is not in dispute, it is nevertheless evident, that great impor-

tance must be attached to the rapidity of its disappearance, when the question of its supply to the tympanum is under consideration, as slow abstraction only requires slow renewal, and rapid abstraction an equally rapid supply. For the purpose of placing this important function of the mucous membrane prominently before the mind, so that due importance may be attached to the method of supplying the absorption thus continually going on, the following experiment is given, viz.:

Fourth Experiment.—First, find the utmost hearing distance from the watch, aided by the thread in measuring as before mentioned; note this. Second, inflate the middle ear by the Valsalvian method, thus forcing, by a superabundant supply of air, the membrana tympani away from its normal position, dragging with it the ossicula auditus; immediately after this inflation (refraining from performing the act of deglutition), again note on the thread the hearing distance. Continue making these observations every five minutes for half an hour.

With those experimenters whose Eustachian tubes are normal, the inflation will decrease the hearing; and with those having an inflamed mucous membrane of this tube, thus lessening the opportunity for air to enter the middle ear, it will increase the hearing; and with those whose tubes allow an excess of air —bordering on patency—it will decrease the hearing; but with either party it will be found that the usual amount of hearing will soon return. As this usual hearing could not return until the membrana tympani and ossicula auditus also returned to their usual position, it is therefore, conclusive evidence that the superabundant air was abstracted by some means, and as the act of deglutition was not performed, it must have been rapidly taken away by the action of the mucous membrane of the cavity

The following are the results of the trial of two of my friends who made this experiment for me:

Belleville, Ill. March, 1872.—Found the distance that I could hear from the watch, which I covered; forced my breath into my ears, holding my nostrils closed. Refrained from swallowing. Heard less, but after waiting three minutes, heard at the first distance. I repeated this five times, with the same result.
Mr. J.—

Lebanon, Mo., April, 1872.—I heard my watch 38 inches, then inflated my ears and heard it 28 inches, did not swallow, but in about two or three minutes (a long time when looking on

the watch), I heard at 38 inches more distinctly than at the start. Why is this? Did not repeat it. W. E. L.

Many others made the same experiment, and arrived at the same conclusion, that is, the usual hearing soon returned without the act of swallowing, although a strong desire to swallow was experienced by every one of them.

From the result of this experiment it is evident that air absorption from the tympanum is not a very slow process.

We have other means of measuring the rapidity of the air absorption in this cavity. For this purpose I will relate, as briefly as is consistent with the importance of this subject, the observations made on the following patients having Eustachian tubes closed by acute tubal catarrh, viz. :

CASE XIV.—Master Freddie H., aged about nine years. Examined first on July 25th, 1866. Has suffered for about three weeks from a very severe cold in the head; his throat was also much affected; tonsils enlarged; breathed entirely through the mouth. Supposed cause, prolonged bathing. Hears the watch in both ears only on slight pressure; has considerable pain in the ears, and under them, toward the throat. The membranæ tympana of both ears were very concave.

Attempted to inflate the tympanic cavities by the Politzer air douche, but failed; also failed on the 26th, but on the 28th was successful in opening the left tube, increasing the hearing in this ear to 12 inches (normal hearing distance of the watch about 96 inches).

Aug. 3d.—Hearing to-day same as before treatment; was again unsuccessful in inflating either cavity.

Aug. 4th.—His mother accompained him to-day, stating that his improved hearing of last Saturday did not last until he got home. Was again unsuccessful in inflating the tympanic cavities.

Aug. 6th.—To-day opened both tubes by the air douche. The hearing was not examined before the treatment, but this inflation increased it in the right ear to 13 inches, in the left to 15 inches.

Aug. 7th.—Watch heard with both ears only when nearly in contact. One successful air douche increased the hearing in right to 9 inches, left to 11 inches. Three other inflations to 14 inches on right side, and 17½ inches on left.

Aug. 9th.—Hearing in right 2½ inches, in left 4 inches. After inflation of middle ears by seven air douches his hearing was increased to 18 inches in right, and 22 inches in left. This was about 10:30 A. M. He did not leave the office until 12 M., when, on examination, his hearing with right ear was reduced to 7 inches, left 10 inches. Here was a loss in one and a half hours on the right side of 11 inches, on the left 12 inches. Three more douches increased the hearing to 21½ inches and 27 inches, respectively.

Aug. 10th.—To-day there was a loss in two hours of 13 inches in right, and 18 inches in left.

Aug. 11th.—Loss to-day in one and a half hours 7 inches in right, and 13½ inches in left.

About the same amount of loss of hearing followed the next four treatments; this was also noticed by father of the patient. The observation of this decrease of the hearing so soon after the treatment was first made by the mother of the patient (who was a teacher in one of our public schools); and it was her remarks about it, that were the cause of its being noted by me.

Case. XV.—The next case in which I took notice of the degree of hearing lost after treatment, was my niece, Miss Lizzie I. R., aged ten years. Her tonsils were very much enlarged. Hearing watch with right ear, contact, left 1 inch. The first inflation caused a very loud crack in her ears, frightening her very much. After this air douche she heard the watch with the right ear 18 inches, left 32 inches. I tried for about half of an to pursuade her to allow me to make another application of the air douche, but in vain. Her hearing at this time was reduced 11½ inches on the right side, and 19 inches on left, a loss during this half hour of 6½ inches in right, and 13 inches in left. Four days afterward—March 9th, 1867—her hearing on right side was, contact, left, 1½ inches; after one air douche, right ear 17 inches, left 35 inches, after another, right 22 inches, left 46 inches; then was compelled, on account of the very great dread of the effect of noise that the inflation caused, to desist until 2 P. M., when her father came to my assistance. After she had been away nearly four hours, her hearing had decreased to 12 inches on right side, and 27 inches on left, a loss in this time of 10 inches on right, and 19 on left.

The history of quite a number of similar cases could be

added, demonstrating the rapidity of the absorption of air within the cavity of the tympanum.[1]

If the "deglutition method" of renewing air to the tympanum is correct, the experimenter that refrained from performing the act of deglutition for two hours, must have been as deaf as either of those cases having tubal catarrh, two hours after their treatment, for neither, according to the Toynbee theory, had any means or opportunity of supplying air to the middle ear, in the place of that abstracted by the mucous membrane of the cavity. This should have caused an equal loss of equilibrium in the tympanic air density in both individuals, consequently an equal loss of hearing; but the results prove that those having normal Eustachian tubes did not suffer loss of hearing, by reason of this so-called closure (3d Con.); whereas, those cases having abnormally closed Eustachian tubes, did seriously suffer loss of their hearing, even after a much shorter period of closure.

The Fourth Experiment, and the observations made on cases XIV and XV, prove that even if the act that is claimed to open the Eustachian tube was performed every minute during life, it would not be sufficiently "frequent" to maintain the requisite uniformity of air density in the cavity to admit perfectly uniform hearing. A continuous air absorption necessitates a continuous renewal. Either you must stop the continuous abstraction, or you must allow the continuous supply. The alternative is physically unavoidable.

Again, I deem it self-evident that a uniform tympanic air density implies that there *must* be a uniform condition of the Eustachian tube; therefore, this passage must be uniformly open, or uniformly closed. It is impossible that it can be closed the larger part of the time, and open at accidental intervals, and there exists at the same time a uniform air density in the tympanic cavity while air absorption is continuous. Now, if the walls of the Eustachian tube are constantly in contact, or only in contact during the interval between the acts of deglutition, in what condition will the air that remains in the cavity be in respect to its density, as compared with that on the outside? Most certainly it will soon be in a rarefied condition (4th Con.), unless it can as freely and continually enter the cavity as if it had an

1. I have observed the hearing decreased in like cases at the rate of from 18 inches in two hours to 16 inches in five minutes.

open tube to admit it. Even if the "deglutition method" of tympanic ventilation is correct, much the greater period of time, this air in the cavity must be in a rarefied condition, as the absorption is rapid enough to produce this effect upon it, and the act of deglutition is not made sufficiently frequent to prevent it. From this it appears as though there would be a necessity for a patency of the Eustachian tube—that is, if the normal air density in the middle ear must—as asserted by Toynbee, Tröltsch and others—be maintained equal to that of the surrounding atmosphere; but instead of this, there is, in my opinion, a physical necessity for a rarefied condition of the air density in the middle ear, even if the Eustachian tube was only closed for a short period of time, to allow the oscillations of the membrana tympani to move with greater freedom, and to prevent undue condensation of air in the tympanum by the vibrations of the membrane, occasioned by the waves of sound.

Article XIX.

On Some Points in the Pathological-Histology of So-called Phthisis Pulmonalis, or Consumption. By Z. Collins McElroy, M. D., of Zanesville, Ohio.

Mortality reports of the past year show one-fifth of the total deaths in several large cities, is due to what is called "consumption." And including pneumonia and bronchitis, one-fourth of the total mortality. As near as I can ascertain, in the absence of any reliable statistics, that is about the ratio of mortality in our own city from consumption, pneumonia and bronchitis. I do not think it will vary much from that anywhere in our county. Perhaps it is less in the Southern States, and greater in New England, but still not disturbing the average for the whole country.

A study of the main condition, to-wit: consumption, must always be a subject of profound interest to physicians.

My own understanding of what is consumption, is this: A

waste of the body, at a higher than natural speed, without being balanced by repair. In the beginning, without noticeable local complications, but as it progresses, the failure of structure in the lungs becomes prominent, though the waste of all parts of the body keeps pace with that of the lungs.

I do not propose to reproduce the symptomatology, as it would, it seems to me, be a waste of time, as it is contained in most text-books, and in various special monographs, much more complete and systematic than would be proper on this occasion.

And for precisely like reasons, nothing will be said of post-mortem appearances with unassisted vision, and microscopic sections could only be interesting enlarged on screens.

And for the further reasons, that nothing can be learned, not already known, by a study of the forms, colors and other physical features of structure in a condition of decay: For, with every more or less complete change, or modification of physiological forms of structure, there are modifications of, or total abolition of function.

I am not an admirer of Rev. Joseph Cook, nor a constant reader of his "Monday Lectures." It is very seldom I read one at all. But that delivered by him on the 20th of December last (1879), on the anatomy and functions of the brain, is exceptionably fair in its resumé of what is known on the subject. I mention this now only for a single object, which I think he has very happily formulated. It is thus stated: "Every function performed by a living body, has its correlate in structure." These are not exactly his words, but they are, to my understanding, just a little more explicit. I have on various occasions presented the same general truth in other words to our medical societies.

My own formulation has been as follows: "Every function performed by a living body, has behind it special forms of structure on which it depends." Any change in any structure is absolutely certain to be followed by a corresponding change of function.

There is one point on which hinges the whole pathology of the condition of the bodies which we call consumption. It is a histological point, and has not, up to this time, engaged the attention of experimenters, and observers. It is this: by what means, exactly, is food converted into organic structures during life; Or, it may be stated thus: "How does food become living tissue?"

If the answer is "by digestion and assimilation," let me ask are we any wiser than before? What is digestion? What do physiologists mean by the term? What do they mean by assimilation? Is it by some orderly process which can be followed from beginning to end and understood? Or, is it by some hocus pocus, haphazard process, which cannot be followed or understood? The former is my understanding.

Confining our investigations to the complexities of animal—higher animal—life, it does not seem to me possible to get even a clue to it. But turning to the simpler forms of organic life in the vegetable kingdom, a clue can be obtained that will not be misleading.

Study attentively the mode of reproduction in any of the more common forms of vegetable life around us. They are reproduced, and the means are not a secret, even to the most careless observer. How is grass, for instance, reproduced? The proper reply is by a seed. And what is a seed, say of grass, wheat, corn, rye or oats? It is the material in which the parent plant has *stored up* the material and force for a two-fold purpose, viz.: the necessary material and the requisite modes of force to form the structures of the new plant, up to that point in its growth and development, at which it can appropriate other and new material to its further evolution.

Grass, or other vegetable forms of organic life, when the means for their multiplication and preservation have been perfected, that is, when the seed is ripe, so to speak, the parent plant is dead. The seed falls to the ground in the cases under consideration, amidst the debris of the decaying parent plant; and from the midst of this decaying mass, with favorable external conditions, comes forth the new life and the new plant or plants. Analogy, not always a safe guide, is not likely to lead us astray in this case, and will be sure to show the investigator that the same law holds good in the complex animal, as in the simple vegetable and may be formulated thus:

"Each special histological structure in a complex living being, in the act of physiological decay and the performance of function, stores up, in the requisite material, the necessary force for its own reproduction from other and new material."

If the structures of a living being at all times did this perfectly, and new material and necessary conditions were always present, and the processs of reproduction always perfect, man

would be immortal; would live in a state of perpetual youth. This is so far from being the actual state of the case, that, after maturity, the years will leave their traces on all the structures, and it is by this gradual loss of perfect physiological structural forms that men grow old and pass away. Advancing age is the correlate of failing tissues.

But it will be instructive to follow food, so far as it can be traced in its pathway to living tissue. All the food ever eaten by a living being, and I have man in my mind prominently in my present study—is germinal—that is, it is the material in which some previous existences have stored up the material and force for their own preservation and multiplication. And as we are not cannibals, none of us ever partake of food in which a being of our own kind has stored force for its own preservation.

Chemically, the food eaten by human beings is divided into the carbonaceous and nitrogenous. Another classification is by proximate principles, viz.: starch, fat, sugar, albumen and fibrin, gelatin, etc. Associated with our food, as we get it, there is always more or less woody fiber and other insoluble matters.

The stomach may be regarded as a receptacle of food in which it, or so much of it as can be, is dissolved by the aid of acids, and organic principles, the most prominent of which is now understood to be pepsin. The office of pepsin is to dissolve heat coagulated albumens and fibrins. The acids of the stomach dissolve the starchy elements of food. Oils and fats are emulsified. As rapidly as these several proximate principles are dissolved, they are passed directly into the venous blood-vessels, and go forward to the liver. In the liver there is a mingling of old blood—blood from the venous capillaries—and the new material from the stomach. All that takes place there is not understood, but it is very certain that the starchy compounds are converted into glucose—grape sugar.

The stumbling block in the way of physiological investigation has been, and is now, the lacteals. Current physiology assigns to them the functions of "absorbing" certain products of digestion of new material—food—by the stomach? Why so? The structure of the lacteals and lymphatics here and elsewhere are identical.

The lymph in the lacteals, during so-called digestion, is in larger quantity and differs from lymph in the lymphatics elsewhere, only in the greater amount of fat emulsion. And why should

not the lymph be greater in the so-called lacteals during digestion—that is, solution of new food in the stomach—than at any other time? Does not increased functional duty include increased physiological decay of structure performing it? Surely this is so. Increased duty means increased decay of structure in the intestinal tract, including in this its whole extent. Then, if the structures store up the force in the necessary material for their own reproduction from new material, there would of necessity be a larger amount of lymph in the lacteals during their periods of functional activity, than in periods of functional repose.

In truth, the function of the lymphatic system would seem to be to gather up the material in which the structures have stored up the force for their own repair and perpetuation from the general debris. The lymph, therefore, is the analogue of a vegetable seed. It performs in the living body precisely the functions of a vegetable seed in the vegetable kingdom. To adopt it to the performances of its function continuously during the life of any individual, it must be mingled with the necessary new and old material exactly as it is mingled in the human body. And, further, it must be exactly as it exists in the human body to perform this function. And that is, these several things, old material, new material and the lymph—the seed of the various structures from which it has been derived—must be mingled together just before coming in contact with the gaseous atmosphere, containing ozonized oxygen.

The mechanical arrangement of these several parts may be varied, but the essential principle must be strictly adhered to, as it is throughout animated nature.

The researches of Klien, in London, have demonstrated the identity in histological structure of the serous membranes and lymphatic ganglia and glands.

But to return to the stomach. It will be seen that a truer physiology will not regard the lacteals as carriers of new material at all. All new material, after solution is effected in the stomach, is sent through the venous blood-vessel system to the liver direct. Here it is mingled with the old blood returning to the lungs from all the capillaries.

When the changes effected by the liver, in both new and old material, are completed, it is sent forward to the right heart. Just before it drops into the right auricle, the thoracic ducts pour their contents into the stream, and it goes thence to the

lungs. In the lungs, this stream—old blood, new material and lymph, or seed of the various structures—meets the gaseous atmosphere. Some of the changes occurring there are known, but probably much that does occur there is still to be made out. But whatever they have been, all identity of its constituents are lost, and the outgoing stream—arterial blood—is at once, like the contents of the egg, the material and force to reconstruct the wasting tissues in the arterioles and capillaries. It will be seen that whatever there is of *digestion*, if by that term it is meant to cover the changes necessary to convert food into living flesh, must of necessity take place in the lungs.

The stomach may do its duty perfectly; the liver ditto; the lungs ditto; but if the structures have failed to provide for their own repair from new material, or the seed-gathering apparatus—the lymphatic system—fails in its duty, there must of necessity be a failure in the repair of momentarily wasting tissues, at least with full physiological capacity to perform function. The subsequent chain of events would be waste of tissues—not alone in the lungs—without being balanced by repair from new material. And that, it seems to me, brings to light some of the hidden mysteries of the pathology of so-called phthisis pulmonalis, or consumption.

Difficulties in the performance of the functions of the stomach end of the assimilative apparatus are well known as dyspeptic, and are much better understood than those at the other end, where food becomes living tissue. Dyspepsia has its phenomena in pain and uneasiness, but once the food gets past the stomach, it is converted, without further unpleasant symptoms, into living flesh. The failures at the other, or capilliary extremity of the assimilative apparatus, have no symptoms of pain or uneasiness. The failure has another language, which has never been properly interpreted. So-called consumptives often have good appetites, good *digestion*, so-called, in stomach—that is, food is dissolved properly in the stomach, and is properly sent to the liver, but the waste of body goes on, notwithstanding. The defect is made known by the progressive waste of body unbalanced by repair from new material.

One other fact in regard to consumption is about receiving its final proof—that is, its self-limited character. And the additional fact, that the progress of the changes in structure are

often arrested, and remain in abeyance longer or shorter intervals, sometimes through life.

These facts explain the so-called successfully treated cases, and the action of so-called remedies which get credit for doing that in which they, in reality, have taken no part.

Thus viewing the pathological condition, in so-called cases of consumption, my expectations of more successful remedial measures than any now known, it seem to me, are well founded. But it will not consist in any specific mixtures of drugs to cure the patient outright. But it will consist of at least common sense in life's surroundings, and less haphazard exhibition of drugs. It seems to me that the blood of persons in good health, of not dissimilar age, and the same sex as the patient, will in some way not now understood, be employed to furnish the force that will perfect the final process of living flesh formation in the capillaries of those in whom this process is deficient, and for the want of a better name we call consumptives.

This same conception of what must and does actually take place in all living beings during life, when applied to other departments of pathology, fits equally well as in so-called consumption. I apprehend in it are comprehended every possible pathological condition; acute and chronic, abnormal, deformed, and monstrosities. For, if the processes of living tissue making from food are not interfered with by some superior force, there could not be any pathology. All life would be immortal and perfect in types and forms.

August, 1880.

ARTICLE XX.

CYNANCHE TONSILLARIS.* By H. V. FERRELL, M. D., Carterville, Illinois.

There seems to be much difference among our authors as to what is understood by the terms cynanche tonsillaris, acute tonsillitis or quinsy. The dictionaries give them as synonymous expressions. Flint in his late work on clinical medicine says that acute tonsillitis or quinsy is an inflammatory affection of the tonsils, ending generally in suppuration. If he means to limit the application of the term to an inflammation of the tonsils and that alone he is perhaps correct in saying that the disease generally ends in suppuration. If however, the term is applied to an acute inflammation of the soft palate, tonsils and fauces so common in the winter as the result of cold—an application given it by some authors—he is not correct in so saying, as suppuration in such cases is a rare occurrence.

This pathological condition of those parts is considered, by him, in what he terms acute pharyngitis with tonsillitis coexisting. In his description of acute pharyngitis we are left to infer, however, that an inflammation of the tonsils is not an essential or even an ordinary component.

In acute pharyngitis, as we observe it in this country, that is taking Flint's view of that disease, inflammation of the tonsils is almost always present. Indeed, I do not remember ever to have seen an acute inflammation of the tonsils that did not at the same time involve, more or less, the neighboring structures, and *vice versa*.

From West, on the diseases of children, 6th ed. p. 479, we quote the following: "Inflammation of the soft palate, tonsils and fauces, constituting cynanche tonsillaris, is not strictly limited to any age, nor attended with any special symptoms when it occurs in the child. It is, however, comparatively rare under twelve years of age, and is almost always less severe than at or after puberty, while I scarcely remember to have met with it

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under five years of age, a circumstance which attaches special importance to sore throat in children, since it will usually be found to betoken the approach of scarlet fever, or of diphtheria rather than the existence of simple inflammation of the tonsils."

No one will question the capacity of Mr. West as an observer, nor the truth and candor of his statements. Hence, we must accept the foregoing as truly representing the disease in England where his observations were made. Mr. Tanner, another English writer, expresses substantially the same views, while J. Lewis Smith, the latest and best of American writers, on the diseases of children, has nothing to say on the subject, except a casual reference to something nearly the same under the head of catarrhal pharyngitis.

The object of this short paper, which might perhaps more properly come under the head of diseases of children than under that of the practice of medicine, is to call attention to the diversity of opinion among authors as to what is meant by the terms cynanche tonsillaris, etc., to its extensive prevalence among children of all ages, and to a brief consideration of its diagnosis, prognosis and treatment.

I have seen cynanche tonsillaris, accepting Mr. West's definition of that term, in children of all ages from one year upwards. I am not speaking of that chronic form which leads to permanent hypertrophy, usually dependent upon a strumous diathesis; but of an acute inflammation affecting the soft palate, fauces and tonsils, causing such a degree of enlargement of them as to give rise to dyspnœa and dysphagia and in extreme cases to almost complete loss of the power of speech. During the present winter it has prevailed very extensively within the bounds of my practice. Scarcely a single family of children but what one or more were affected, and in many cases all of them, as in my own family. It usually seems to be caused by exposure to cold, and when once it affects a patient it leaves them ever afterwards much more liable to its attacks. In its diagnosis the special importance which attaches to sore throat in children, alluded to by Mr. West, should never, for a moment, be lost sight of. Prof. Gregory's rule is a good one: "Always regard a child as being in danger, when it has anything the matter with its head or throat." "Look out for squalls, for if the child don't squall, the mother may."

We must remember that the condition of things described is no prophylaxis against croup or diphtheria, and as croup is in the majority of cases excited by cold, it may readily supervene upon cynanche tonsillaris, as I am sure I have seen it do. Usually there is not much febrile disturbance, often the first indication of the child's ailment is a somewhat hoarse suppressed cough, disturbing, but not waking it from sleep. The cough has not the dry, brassy hoarseness of croup. Frequently as the child coughs it clutches its throat, indicating the existence of pain there.

On examining the throat externally the enlarged and painful glands may be felt near the angle of the jaw. Examining internally we find the mucous membrane of the soft palate, the fauces and tonsils red and swollen; the tonsils enlarged, in some cases so much so as to almost or quite approximate each other, and sometimes the mucous membrane over them covered with a follicular secretion as in the adult. From croup it is to be distinguished by the absence of high febrile excitement, great dyspnœa, the character of the cough, and absence of the croupous membrane.

The prognosis as regards the cynanche tonsillaris itself is always favorable, terminating as before stated either in resolution or chronic hypertrophy, no case within my knowledge in children having proceeded to suppuration. An important point in the prognosis, and one about which the physician is most sure to be asked, is the liability to the supervention of croup. As already observed this disease is no bar to the development of croup, but whether it conduces to the development of that dreadful disease or not, I am not so well prepared to say. Does the same cause or causes which produce the one, produce the other? If so, does cynanche tonsillaris merely mark a milder degree of the action of the common cause or causes? This hypothesis is open to the objection that should it be correct we would be likely to have croup developed more frequently, whereas the fact is it is a very rare occurrence. Does cynanche tonsillaris stand in a causative relation to croup? If it did, it would be very unlikely that whole neighborhoods of children would be affected with the former and none with the latter. From these considerations we are disposed to regard croup as being dependent upon a specific poison in the system, as in the eruptive fevers, and cold

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merely as an exciting cause, and the existence of the two diseases in the same patient as merely a coincidence.

In the treatment of any disease whose intrinsic tendency is towards recovery the therapeutic effect of all remedies must be more or less problematical. I have frquently seen cases of this affection pursue their course to recovery without any treatment whatever. Notwithstanding this I believe that the course of the disease is favorably affected by treatment. That which I have found most efficacious is to first wash the mouth and fauces well with a saturated solution of chlorate of potassa—using boiling water—then give a tablespoonful or two of the same solution, into which just before giving, is dropped from three to ten drops of the muriated tincture of iron. Do this every two, three or four hours, as may be required. Warm foot baths; externally applied over the enlarged tonsils, the compound tincture of iodine. Quinia with hyd. c. creta is required in some cases. Iodide of potassium three times a day in doses suited to the age of the patient, I think both hastens the cure and prevents chronic hypertrophy.

ARTCLE XXI.

GUNSHOT WOUND OF THE HEAD.* By J. T. BINKLEY, M. D., of Shawneetown, Illinois.

Sunday, February 23, 1879, Joseph Barger, colored, æt. 23 years, received the discharge from a Parker Bros'. shot-gun, gauge twelve, loaded with No. 4 shot (heavy charge). The muzzle of the gun when discharged was six or eight inches from his head. The load penetrated the skull one and a half inches anterior to the lambdoidal suture on the left side, near the posterior margin of the parietal eminence, following it two inches and one-fourth to a line drawn from one auditory foramen to the other, and about three inches above the left external auditory canal, shattering, depressing and carrying away soft parts, bone, membranes and cerebral substance.

The soft parts were destroyed three and a half by one and a half inches, and that of the skull two and one-fourth by a little over one inch.

Judging from the history of the case and the appearance of the wound, I would guess the range of the gun to have been about forty-five degrees.

He was shot about 9 or 10 A. M., fell, and lay upon the cold ground some two or three hours apparently unconscious. He was then conveyed to a farm-house near by, still remaining as before. I arrived at the house at 3.30 P. M.; found him covered with a cold, clammy perspiration; the extremities very cold; pulse 28; respiration 20 per minute; unable to move either right extremity, or articulate a syllable. The extremities were perfectly passive, and the entire right side totally paralyzed. On a more extended examination, I found his apparent unconsciousness was the result of aphasia. There had been considerable hemorrhage and it was still going on, but to no alarming extent. Brain substance was oozing from the wound with the blood.

My examination was partial, as I at that time considered the

*Read before the Southern Illinois Medical Association, held at Cairo, Ill., January, 1880.

case necessarily fatal. I however ordered mustard and hot applications to his extremities, cloths wrung out of ice water to his head, together with alcoholic stimulants, until reaction was established.

February 24th.—Still alive; reaction fully set up; pulse 84; respiration as before. Moved him six miles—one by land, five by water—the shaking of the boat caused pain of the head, manifested by groaning and placing his left hand to his head; now answers questions by slight shake or nod of the head. Complains exceedingly of his head; continued cold applications and ordered five grains bromide potass. every two hours; beef tea and essence for nourishment as required.

25th, A. M.—Feels comfortable; pulse 68; respiration 30; has passed urine two or three times. Midnight: is a little restless; pulse increased; tongue coated; loss of appetite. Ordered: Hyd. chlor. mit. grs. x. Pulv. doveri, grs. xii. Ft. ch. No. iv; S. One every three hours. Continued local treatment.

26th—A little flighty, yet recognizes friends; pulse 60; no change in respiration; bowels freely moved; can now articulate yes or no, and tries to talk. Head pains him; no appetite; tongue still coated. Repeated mercury with one-third grain doses of morphia every three hours, also the bromide continued. Local treatment as before.

27th, 8 A. M.—Rested well during the night; pulse 50; respiration 16. Dressed wound, found it suppurating slightly. Scalp around the opening slightly swollen; brain protruding, and the whole space pulsating; continued dressing. 6 P. M.—pulse 34; tongue cleaning; appetite better; has taken beef tea, two soft boiled eggs and two crackers.

28th—No change except slight headache; was a little restless during the night. Ordered potass. brom. grs. x every four hours.

March 1st, 8:30 A. M.—Rested well during the night; pulse 60; respiration 16. Tingling sensation in toes and fingers of right side. 8 P. M.—Increased heat and headache, tongue dry and red. ℞—Hyd. chlor. mit. grs. x. Pul. doveri, grs. xii. Ft. ch. No. iij. S. One every three hours.

2d—Pulse 56, soft; respiration same; feels and looks better; bowels and bladder evacuated. Changed clothing; dressed wound, found it doing tolerably well; applied cloth to the wound, dipped in following: ℞—Oil olives ℥j; acid carbolic gtt. xv; then folded

a cloth several thicknesses and applied it saturated with the following: R—Tr. arnicæ aquæ puræ aa ℥j, with ice cap over the cloth.

3d, 8 A. M.—Pulse 80; respiration 20; a little restless during the night; no change in treatment. 1:30 P. M.—pulse 88; respiration 26; very drowsy; wound has an offensive smell, parts around the wound œdematous and erysipelatous in appearance; increased carbolic acid; other local treatment continued.

4th.—No change in pulse; ordered seidletz powder. No appetite; coated tongue; ordered calomel, dovers powder and nitrate potass. aa grs. x; ch. No. iij. S. One every three hours.

5th.—Rested better during the night; pulse 104; respiration 20; made water freely; speech improving, speaks indistinctly and slowly; had some fever during the day. The bromide has been discontinued since the stupor. 8 P. M.—Pulse 108; respiration 22; ordered: R—Fl. ext. gelsemium ℥j; v. viride ℥ss; spts. niter ℥ss.; syrup aurant. ℥j. M. S. Teaspoonful every two hours.

6th, A. M.—Rested better; pulse 84; respiration 18; fever abated; speech improving; can now for the first time partially protrude his tongue; appetite better; eats plenty; secretions improving and discharges normal. Treatment continued.

7th, 7 A. M.—Called early on account of hemorrhage. On examination found it was from the scalp and easily controlled by compression. Pulse 80; respiration 20; articulation continues to improve; protrudes tongue full length. Appetite restored; wound healthy and beginning to slough, hence the hemorrhage. Complains of soreness and occasional darting pains in right extremities, sensation returning. The offensive odor about the wound improving; slight suppuration; pus healthy; œdema subsiding. 5:30 P. M.—Has had contractions of the muscles of the neck and back, pulling the head back. Occasional spasmodic contractions of these muscles, causing the head to be jerked back. Increased dose of gelsminum to five drops every two hours.

8th, A. M.—Muscular trouble relieved; pulse 82; respiration normal; rested tolerably well during the night. No change in treatment. 10 P. M—Was summoned to see patient, found him a little restless, the result of too much food; had eaten oysters, eggs, toast and milk. As he had had free evacuations from his bowels and slight increase of heat, I gave the following: R—

Potass. nit. grs. viii; pul. opii. grs. jss.; trit. div. ch. No. ij, S. One every four hours; dose of gelseminum decreased; constant itching of right leg and foot.

9th, A. M.—Patient improving in every respect; changed wash for wound to bromo-chloralum one to eight, to be applied three or four times a day.

10th to 13th—Pulse range 72; natural evacuations; appetite good; takes fresh oysters, beefsteak, eggs, milk, etc.; articulation improving; gave detailed account of his misfortune, what transpired whilst he lay in the woods, apparently unconscious. No change in treatment.

14th—Pulse 70; tongue slightly coated; has sore, tired feeling; sits propped in bed to rest himself during the day. At night gave calomel and dovers powder aa grs. x, at two doses four hours apart.

15th—Still improving; has had slight cerebral hemorrhage, which stopped of its own accord.

16th, A. M.—No change; bowels moved twice; slight cerebral hernia, about ¾ of an inch in diameter at the anterior margin of the wound. 7 P. M.—Pulse 80; has eaten too much, and pays the penalty; heaviness of feeling and increased circulation; pain in head; pain in right leg and knee, with twitching of the muscles of same; moves the toes on right foot.

17th, 18th—Continues to improve. Changed wash to head to acid carbolic gtt. xvi, to ℥j water. Involuntary movements of both right extremities.

19th, 20th—Still gaining; can flex and extend both right extremities by the assistance of the left; sits up and smokes pipe; articulates most words perfectly.

21st to 30th—Has steadily improved, and is now able to walk by being supported on the right side.

April 1st to 5th—Wound has cleaned off and exposes some depressed bone heretofore hidden. During the rest of the month has continued to improve in every respect. Still has pain and involuntary contractions of the muscles of the right arm and leg. Is able to walk about town, with the assistance of a cane.

May 1st—Patient's appetite, pulse, respiration and discharges all normal; gaining strength rapidly; wound closing; protruded brain receding. To-day removed from posterier part of wound a spiculum of bone one by three-fourths of an inch in extent.

27th—Removed two pieces of bone, one fourth by five-eighths

of an inch in extent; others becoming elevated and can be felt with probe.

28th—Removed from posterior part of wound a piece three-fourths by five-eighths of an inch in extent, and half the thickness of the skull.

30th—Removed three pieces, one larger than any of the former. Slight hemorrhage followed but soon subsided without interference. Since the last operation pulsation is very perceptible. When these spicula first made their appearance they were wedged down in a V shape, the outer margin being retaind by the lower edge of the inner table of the skull. Occasional pulling at them with the forceps soon loosened them so that they were easily removed.

A few weeks later I removed several small pieces of bone and one shot, which seemed to have been driven under the anterior margin of the lower table of the skull into the brain. They appeared to have made their way back with the pus.

His friends at different times have removed some small pieces of bone and one shot. Altogether there have been from ten to twelve pieces of bone and two shot removed from the wound.

There is still a considerable depression, but it is covered with integument and hard, unresisting substance beneath. The contraction of the cicatrix has been so great that the hair comes within the margin of the opening in the cranium.

Speech has entirely returned, together with fair use of right arm and leg, both of which are still improving.

The case may be regarded as one of interest, as it locates the part of the brain disturbed and the phenomena following, viz.: For a time total paralysis of the right side and loss of speech; the gradual return of articulation, sensation and locomotion.

These preceded first by a tingling sensation not unlike what follows the pressure of a nerve, secondly acute pain in the paralyzed limbs; third, involuntary twitching of the muscles; next, moving of the fingers and toes; finally, their restoration.

In the treatment there is nothing materially new, yet I am satisfied that gelseminum is not generally used in treating cerebral disturbances. After the adoption of it in this instance, I invariably found all head disturbances relieved, as soon as I could bring my patient under the influence of the drug. This I did by commencing with a small dose and increasing a few drops at each time until my object was accomplished.

Article XXII.

Tracheotomy Performed for the Extraction of Cockle Burr.* By G. W. Norman, M. D., Smith's Mills, Ky.

October 23d, 1879, at 10 o'clock, A. M., was called to see Katie Porter, aged 14, who was reported to have gotten a cockle-burr in her windpipe. Upon arriving at the house of Mr. Porter, a distance of 4 miles, I found his daughter suffering excruciating pain in the region of the trachea, with breathing a good deal like a case of laryngismus stridulus. I obtained from her father the following statement:

He said his daughter was gathering beans in the cornfield near the house, and a burr was sticking on her dress sleeve and she attempted to remove it with her teeth, (her hands being engaged), and in so doing coughed and sucked the burr down her wind-pipe; upon physical examination found her respiration varied, at times bronchial in character, and when the dyspnœa was greatest produced a whistling sound, audible all over the room. The patient having vomited freely and made repeated efforts to cough the burr up, and seemed to be so exhausted, I decided at once that tracheotomy would be the most available means; so started a messenger to Henderson immediately, a distance of twelve miles, for medical assistance. At 9 o'clock, P. M., Drs. Hodge and Dixon arrived at my house. Circumstances considered, we determined to delay the operation until morning.

At an early hour the following morning we arrived at Mr. Porter's. After examining the patient, Drs. Hodge and Dixon agreed with myself, that to open the trachea was the only hope, so we proceeded at once to make the opening, Dr. Hodge performing the operation in the usual way, assisted by Dr. Dixon and myself.

The patient passed kindly under the influence of chloroform, the operator proceeded slowly, and succeeded in performing the operation without severing any important blood-vessel, hence, the hemorrhage amounted to but little. When the windpipe

*Read before the Tri-State Medical Society, held at Evansville, Ind., November, 1879.

was opened it produced a violent fit of coughing, which we hoped would dislodge the burr, but did not; so we adjusted wires, devised by Drs. Hodge and Dixon, so as to give free exit to the expectoration and also to the burr. Three hours after leaving the patient, a lady who was sponging off the collection from the wound, found a very small seedless burr sticking to the sponge, which she thought must be the one which the girl had swallowed. Upon my visit at 5 o'clock in the afternoon, after hearing the statement of the lady and being somewhat undecided about its being the burr, and as her breathing was still much disturbed, I decided to leave the wound open for farther development.

October 24th—Called to see the patient, found her doing so well, thought that must certainly have been the burr.

October 25th—Found the patient decidedly worse, with breathing very much disturbed and considerable inflammation in the larynx, extending low down in the trachea; and upon physical examination, found present large bronchial râles, also high fever and pulse 120. I gave the patient about one-sixth of a gr. of morphia, with directions to repeat the dose if she did not rest more quietly. Upon my subsequent visits found her alternately better and worse, with more or less fever, and when I attempted to close the opening, found that it obstructed her breathing so much, determined to leave it open until the inflammation entirely ceased.

October 30th—Six days after the operation, she had a violent fit of coughing, and a lump, seeming to be blood and mucus, lodged in the hole in the trachea, and upon removing it, it was found to be the burr proper, which I now have in my possession.

Her father stated that her breathing was very much relieved as soon as the burr was expelled.

Examination still revealed mucous râles, which were not so intense, pulse 95 to 100, with but little fever; brought the edges of the wound together with adhesive strips, the patient breathing comparatively well through the mouth.

October 31st—Found patient still improving, and has continued to improve steadily ever since, and at this writing, Nov. 4th, is able to sit up in bed and eat heartily; breathing entirely through her mouth, the opening in her windpipe being well closed with adhesive strips and healing as rapidly as could be expected.

ARTICLE XXIII.

CANCER OF THE STOMACH.* By S. W. MARSHALL, M. D., of Sparta, Ill.

Very much that is now known of the science of medicine is the result of patient observation at the bedside. Faithful and earnest men have watched the progress of disease, as it lead on to death; have carefully noticed its symptoms during life, and after death have observed its effects upon the different organs of the body, and thus have done very much to clear away the mists that have stood in the way of progress in our noble profession.

If every laborer in the profession, however humble he may be, would conscientiously examine and report every case of death, from obscure and uncertain causes, very much that is now clouded with obscurity would be made clear.

It is this conviction that leads us into the preparation of this paper, rather than to write upon the subject assigned us in your programme. On the latter subject we have no special qualifications to write, nor any facts of interest to communicate. Nor shall we permit ourselves to thoorize, or speculate on the theme which we have chosen, but will confine ourselves, as near as we may, to a simple detail of what we observed in three cases of cancer of the stomach which occurred in our practice within the last two years. The first came under observation on or about the 1st of January, 1878.

Milton M., æt. sixty, of medium build and height, temperate in his habits, and had always enjoyed fair health. No constitutional dyscrasia could clearly be established. Had been sick from malarial fever in September, and had not fully recovered. Was considerably reduced in flesh and strength.

Complained of great difficulty in deglutition, at times very much worse than others. Now and then by exercising great care, he would be able to take food of a liquid or pulpy nature tolerably well; at other times he was utterly unable to take any

*Read before the Southern Illinois Medical Association, held at Cairo, Ill., January, 1880.

thing at all. Thus he was kept in a state of semi-starvation, scarcely getting enough food to sustain life. His appetite was good, his tongue clean; and he had no pain except a slight uneasiness over the cardiac end of the stomach.

Examination with the stomach tube discovered stricture at or near the lower end of the œsophagus, whether functional or organic we could not certainly determine. The fact that he could take food at one time, and then was utterly unable at another time, inclined us to the belief that it was possibly functional, or spasmodic. Under tonic and anti-malarial treatment, he improved some for about two months. After this our efforts were of little avail. Deglutition grew more and more difficult. Day by day he lost flesh and strength. The skin began to assume a dirty, waxy and cachectic appearance.

At this time our patient, utterly discouraged with the futility of our efforts to relieve him, ceased for a time all medication. We saw him next at his house on the 15th of August, and found him in a most pitiable condition. For three days he had been utterly unable to take the slightest particle of food. Every attempt at deglutition produced strangulation and regurgitation, always attended by the ejection of a thick, soapy and exceedingly tenacious mucus. He was suffering the pangs of extreme hunger and thirst.

As our only available resource, we advised the use of nourishing and stimulating rectal injections. They were well retained, and for a time, he gained strength; got up, and after a while was able to take small quantities of liquid nourishment by the mouth. This temporary improvement continued, sometimes better and sometimes worse, until about the first of December. After that time he grew steadily worse, and was almost constantly confined to his bed.

From this time, and until the end of his life, little nourishment was taken by the mouth, or retained when given by injection. There was considerable increase of pain over the cardiac end of the stomach. No tumor was at any time discovered.

He died on the 16th of January, 1878, literally of starvation. Post-mortem examination, thirty-six hours after death: We were kindly assisted by Drs. Burges and Arnett. Rigor mortis well established; stomach and bowels empty. All of the tissues in an extremely emaciated and bloodless condition, but otherwise healthy, except at the cardiac end of the stomach.

There we found a hard, nodulated and evidently malignant tumor, one and a half inches in diameter, invading the tissues of the cardiac, and almost completely occluding the entrance into the stomach. There was very firm adhesion between the tumor and surrounding structures.

The œsophagus was not involved. Our second case came first under observation November 18th, 1879. Robert M., æt. sixty-eight, had up to about three and one half years prior to this time, enjoyed a fair share of health and strength. At that time he began to suffer from occasional attacks of indigestion, attended with acid eructation, and occasional paroxysms of pain in the epigastric region. At first the pains were neither very severe nor frequent, and attributed to some trivial cause. About two years before his death, and one year after the advent of the gastric symptoms, he had an attack of pernicious fever under which he nearly succumbed.

From this time he remained something of an invalid and there seemed to be some increase in the gastric symptoms, the pain increasing in severity and frequency. From having a paroxysm every two or three days he began having them every day; then two or three times daily, and sometimes oftener. From a slight uneasiness, it increased to a pain, which caused him to double himself up and writhe in agony. Steadily increasing doses of opium, given to allay the pain, ultimately entirely failed to give relief. In the same proportion the other symptoms grew worse. The desire for food grew less and less.

Only the most palatable and most easily digested food could be taken and that in very small quantities. Flatulence and constipation grew very troublesome; bowels seldom acting without a cathartic. Emaciation deepened into cachexia and the patient became gloomy and despondent. Occasional vomiting now and then attended by the ejection of food or mucus stained with streaks of blood, and after a while composed wholly of disorganized blood. This latter symptom, however, was never very persistent.

During my attendance upon him (the last two months of his life) his tongue was generally clean; there was no thirst and there were no febrile symptoms. Determined pressure over the greater curvature revealed the presence of pain, but discovered no tumor. During the last month of his life his feet and legs became œdematous from dropsical effusion.

Death occurred on the 24th of January, 1879, from pain and exhaustion.

Post-mortem examination by Dr. D. S. Booth, assisted by Drs. Guthrie and Arnett, twenty hours after death. Body greatly emaciated; abdomen contained a quart or more of bloody fluid. Extensive agglutination of bowels, spleen and stomach, by peritoneal inflammation. A considerable portion of the greater curvature was destroyed by ulceration. The edges of the ulcer were thickened, patulous and irregular. The ulcer was three or four inches in diameter, and for some distance beyond it, the walls of the stomach were hardened and contracted and possessed little of their structural integrity. All of the other organs were healthy except the spleen, which was hypertrophied, but most likely from previous attacks of malarial fever. We regret that we were not permitted to take away any portion of the diseased structure for subsequent examination.

This brings us to our third case. A. S. Dickey, a farmer, aged 45, of strong muscular build; weight about 180 pounds. Always enjoyed good health; had one brother who died of encephaloma of the face, another from an obscure pulmonary trouble (possibly malignant), and a third from what his physician diagnosed sour dyspepsia. From the most careful inquiry we have no doubt this brother died of the same trouble as did our patient. The friends who waited upon them both say that their symptoms were identical. Mr. D. had an attack of indigestion, characterized by very obstinate acid eructation about the first of July, 1877. This continued for about one month and then gave way. He continued in tolerable health, suffering a little at times from slight attacks of indigestion until about the first of March, 1878. He then began to complain bitterly of what he called "sourness of the stomach." Everything he took into his stomach fermented and gave him pain. All of the usual remedies to correct this condition were thoroughly and persistently tried in vain, or at least with but temporary relief. These symptoms steadily grew worse, and he began to complain a great deal of pain in the epigastric region, increased greatly after eating. About the middle of May he began to vomit his food, sometimes five or six hours after eating, and at other times not until another meal was taken. This always gave partial relief. At this time his countenance began to assume a pinched and

anxious expression, and that peculiar yellowish tinge so characteristic of organic visceral disease now became very striking. We informed his friends of his hopeless condition, but out of mercy for himself did not, at this time, altogether give him over to despair. Every means known to us was tried to promote digestion and assimilation, but to no purpose. True to the history of malignant disease the course was steadily downward. During the sitting of the Association in Sparta we had the great pleasure of having this patient examined by Drs. Hodgen and Hughes, of St. Louis, and several eminent gentlemen present with us to-day, all of whom confirmed our diagnosis. Strengthened in our opinion by their counsel and feeling that our patient was rapidly approaching his end, we so informed him. After this emaciation and exhaustion rapidly progressed. All desire for food was lost, and on account of the pain it produced little was taken. Constipation of the bowels, always troublesome, grew more and more obstinate. Nourishment per rectum was tried, but could not be retained. A few days before death diarrhœa set in and hastened the fatal issue, which occurred on the 10th of July, 1879, about five months after the advent of the acute symptoms. Post-mortem ten hours after death, assisted by Drs. Booth, Guthrie and Brown. Stomach greatly distended and containing two or more quarts of water. The pylorus was firmly adherent to the surrounding tissues. Its walls were thickened and indurated, and so contracted as to stop all communication between the stomach and the bowels. This was all the lesion found. There was no softening or disintegration of tissue. We removed the diseased structure and sent it to Dr. Hodgen for microscopical examination, but unfortunately it reached his office during his absence in Europe, was laid aside to await his return, and was lost. This we greatly regret, as we feel sure that the examination would have verified the diagnosis.

In concluding this paper we would call attention, in the first case, to the absence of definite diagnostic symptoms, there being for several months none of the characteristic symptoms of malignancy. Also to the freedom from pain as contrasted with the two latter cases. In the second case the prominent element, all the time, was steady, persistent pain, attended with frequent and terrible paroxysms, coming on at any time, and independent of the presence or absence of food in the stomach. The absence of desire for food, rather than the presence of dyspeptic symptoms,

and the steady and progressive emaciation and loss of strength; the age of patient, and lastly the great destruction of tissue. In the last case the chief features to which we would call attention are, first, the clear hereditary taint, the marked cachexia, and the rapidity with which it run its course.

ARTICLE XXIV.

OCCLUSION OF THE ANUS.* By Wm. C. LENCE, M. D., of Jonesboro, Ill.

I was called to see Mrs. H., May 8th, 1879, in confinement. In a few hours after my arrival, a strong healthy male child was delivered, weighing full ten pounds, and apparently all right. The mother was soon made comfortable, the child dressed, etc., when I took my leave, saying that I would call again. I made my second visit, about twelve hours after the birth of the child, and after a few moment's talk with Mrs. H., I then turned to the nurse to make some inquiries about the new-born infant. In reply to my inquiry, the nurse told me that the child was somewhat fretful, and that there had been no action from its bowels. I examined the breast of the mother, found the nipples well-formed, and then said to the nurse that probably the child was not hungry, and that I thought the bowels would be moved soon as it began to take nourishment. I went away promising to call again the next day.

When I arrived, I was met by the nurse, who promptly informed me that there had as yet been no action—that the child had taken nourishment once during the night, but that it vomited soon after and that it refused to nurse any more.

I immediately ordered the nurse to undress the child, and upon examination I found what I supposed to be occlusion of the anus. I proceeded at once to relieve the occlusion by

*Read before the Southern Illinois Medical Association, held at Cairo, Ill., January, 1880.

making an incision. An incision was made about one inch deep without reaching the rectum. I then employed the trocar, following the curve of the sacrum to the depth of about two inches without giving relief.

I then asked that Dr. F. S. Dodds be sent for, and he and I saw the little sufferer together on the evening of the second day after birth, and made another attempt to reach the rectum by the introduction of a trocar to the depth of about two and one-half inches without success. The child began to show symptoms of extreme prostration, so much so that we deemed it unsafe to carry the operation any further.

I called early on the morning of the third day, and found my little patient in great distress, moaning very piteously, and its abdomen for the first time greatly distended and tympanitic.

I ordered the nurse to annoint the abdomen with fresh lard and to keep constantly applied flannels wrung from warm water.

I then informed Mr. H. of the impossibility of carrying the operation any further, adding that the child would be very likely to die in the next twenty-four or forty-eight hours.

I was sent for in great haste on the morning of the fourth day with information that there had been a slight discharge of fæcal matter through the wound made two days previous. I hastened to the house, made an examination of the wound, found the two surfaces glued together and dry. A diaper was then handed to me which I found saturated with a dark or rather muddy urine. The odor was sufficient to establish the presence of fæcal matter. I then ordered the nurse to envelope the penis in a soft linen cloth, with instructions that it should be changed immediately after each micturition.

I called again in the evening, my little patient apparently better; the tympanitic condition of the bowels not so great.

The nurse handed to me for inspection four of the changes, which evidently proved beyond a doubt that fæcal matter was discharging through the urethra with the urine.

The child remained in this condition up to the twenty-fifth day after birth. Passing urine freely, but leaving only slight strains of fæcal matter on the diaper. Also extreme emaciation and the abdomen wonderfully distended.

On the 26th day after birth, while the nurse was making some local application to soothe the little sufferer, terror was struck into all present by the escaping of gas per urethra, and which

continued to escape for several hours, and often with such force that it might be heard a distance of ten or fifteen feet.

After the gas ceased to escape, and the tympanitic condition of the bowels had greatly subsided, fæcal matter began to discharge in great quantities; there were thirteen discharges during the first twenty-four hours, and of the consistency and color usually discharged by children of that age, since which time it has continued to have from three to six discharges each day; not commingled with the urine as was the case the first twenty-five days of its existence, but urine and fæcal matter are passed separate and at different times.

To-day the child is eight months and twelve days old, weighs twenty-five pounds, and a perfect picture of health.

Proceedings of Medical Societies.

ST. LOUIS MEDICAL SOCIETY.

Saturday, May 22d, 1880.

Dr. Bernays—About three weeks ago I operated on a gentleman from Arkansas, for sciatic neuralgia, from which he had been suffering incessantly for six or eight months, being unable to get relief in any way except by hypodermic injections of morphine. He was getting to be a thorough morphine consumer, and the danger of that habit having been represented to him by his physicians, he concluded to come here and seek my assistance. I found a very aggravated case of sciatica, which was distinguished by the peculiaritp that the paroxysms of pain only remitted for very short periods. His leg was covered with thousands of scars from hypodermic injections and he had to take them two or three times a day. I proposed nerve stretching, to which he readily consented. The operation was performed on Sunday—four weeks ago to-morrow—and I invited half a dozen gentlemen to assist me. I cut down on the sciatic nerve immediately above the knee. I did not intend to stretch the whole nerve, because the pain was in the outside of the leg in those parts supplied by the perineal or outside part of the sciatic nerve. On the very first incision, I struck the nerve, lifted it up on my finger, gave it a thorough stretching upwards and downwards and as hard as I could pull, perhaps 100 or 150 pounds, with my right hand and let it back. It healed up beautifully, and the patient was free from pain for the next six days, and slept well. But on the evening of the sixth day he was taken with a paroxysm of pain which was as intense as any of his former attacks had been. He sent for me immediately, and also for Dr. Atwood. We injected some morphine to relieve him. The next day I cut down and removed about one and a half or two inches of the perineal nerve. This was done after the administration of chloral. Fearing, however, lest the pain might not be entirely relieved by this procedure, I cut down upon the external saphenous in order to be sure of cutting off all connection between the lower part of his leg and the brain.

I sewed up both incisions; one of them was about three inches above the knee, the other about the middle of the calf of the leg, where this small branch of the nerve is easiest to be found. The patient returned to Pine Bluff, Arkansas, where he now is. He is cured and suffers no pain whatever. It has been three weeks since I incised the nerve. All those parts of the leg supplied by the nerve which I cut are paralyzed. But he is fully satisfied, and he knew before the operation was performed that those parts of the leg would be without feeling. I also told him that when the nerves grew up together he might perhaps have the neuralgia again. By wearing a tight-fitting shoe the drop of the foot to the outside is prevented to a great extent. Comparing this case with the other in which I operated for sciatica sometime ago, I must confess that in both cases "stretching" was a complete failure. That man on whom I operated some months ago is suffering yet, and this one was not cured until I had cut down on the nerve.

DR. DICKINSON—It may, perhaps, be remembered that I reported a case here sometime ago in which I treated neuralgia of the sciatic nerve by injection of sulphuric ether, and succeeded in relieving the patient. The pain has not returned.

DR. BERNAYS—I used no other remedies than those I mentioned, but when the patient came to me he said he had been treated by electricty, hypodermic injections of morphine, and atropia; that he had been blistered, that all manner of ointments had been used on him, and all kinds of baths; that also injections of ether has been made with only temporary relief, also injections of chloroform; in fact, I believe all the remedies had been exhausted. He had taken large doses of quinine also.

Pathological Specimens.

DR. BERNAYS—The specimen I have here, gentlemen, is one to which I wish to call your particular attention, because I believe it to be unique and perhaps never before described or demonstrated in the history of medicine.

About six weeks ago I was called upon to see a gentleman living in the southwestern part of the city. Upon arriving there I found him suffering very much from dyspnœa. He had an obstruction in the trachea or in the larynx; some little dry cough, and an expression on his features of great distress. Upon examination of all the organs of the body, I found the lungs and

the heart normal, and could discover no pathological state to account for serious trouble. I began to examine into the history of the case. He asked his wife to leave the room, and then said he had been under treatment for syphilis for a long time; that he had acquired syphilis ten years ago, and had gone through all the stages of it; had been under treatment for syphilis under Dr. Meyer, Dr. Hodgen and Dr. Hartz; had been taking mercury and was taking mercury at the time. I found the mercury had done no particular good, and I substituted for it iodide of potassium. He told me he had tried that before, but that it had done him no good. But I determined to give it to him anyhow, and prescribed, at the same time, iron and quinine in such a way that he took one dose of iodide of potassium, two hours and a half after the iron, two hours and a half after that iodide of potassium again. I started him with eight grains of the iodide, and gradually raised it to fifteen grains. He improved rapidly; his voice, which had been hoarse, became clear, and the cough got better; the ulcers healed, but he got to drinking to excess when he went out.

About eight days ago I was called in a hurry to go to a restaurant on the corner of 5th and Elm Sts. to see this gentleman, who had been taken with a hemorrhage from his throat. Upon arriving there I found him very much excited, and learned that he had spit up, perhaps, a pint of blood, and that his respiration was very much labored, as if the blood was running down into the bronchial tubes and stopping the air passages. However, the hemorrhage stopped. The next morning he came to my office and brought me a complete cast of the bronchi and trachea consisting of fibrine, coagulated blood. I was very much surprised, because it was hard to understand how the breathing process went on. He spit a little reddish slime for a while and got apparently well. But on last Monday or Tuesday I was again called in a hurry to the same restaurant, and found him dead. He died in about five or ten seconds from a terrible hemorrhage from the throat. My diagnosis was syphilitic ulcer in the innominate artery, or in one of the carotids. I got permission to perform a *post-mortem*, which I did, and it verified my diagnosis to a nicety. You will find here a direct communication by an opening (large enough to pass a lead pencil through) between the trachea and the arch of the aorta.

DR. HODGEN—I would like to inquire how a diagnosis was made locating the ulcer between the aorta and the trachea.

DR. BERNAYS—It was made in this way: Before I opened the body, I said to Drs. Pollak and Hypes that we would find the ulcer in the trachea, which was in direct communication with as large a vessel at least as the left carotid. I know syphilitic ulcers find their favorite location in the trachea. The first hemorrhage proved to me that there were ulcers in the trachea below the vocal cords, because I knew that the blood which was in the bronchi must flow down the trachea, fill the bronchi in the way it did, and the rapidity of the man's death from hemorrhage proved it must be a very large vessel which was attacked. As to the pathology of the case it can be very well seen in the specimen. A lot of the lymph glands lie on the trachea and the œsophagus; one of these glands ran behind the trachea down in the mediastinum posterior, and it had undergone syphilitic degeneration. It is very clear, as I found on making examination by the microscope, that this syhpilitic tissue had broken down under mercurial treatment. I do not believe that mercury has any more influence on syphilis than chalk would have—and I never did; but I have given plenty of it, and I know the best men in the profession give it, both in Europe and in America. But I just want to state that it is my personal conviction that it does no good.

DR. JOHNSTON—What do you give?

DR. BERNAYS—Tonic treatment. If I were to get syphilis myself I would not take anything. I think it wears out in the course of years by itself—or kills. As I was going to say tumors were formed around the lymphatic glands, which upon contraction of the new-formed connective tissue closely united the arch to the trachea. Then on the breaking down of this syphilitic tumor, the ulceration from the trachea approaching this place, the pressure of the blood in the aorta aiding it is very easy to understand how a communication formed. It is also easy to understand how the first hemorrhage did not prove fatal, because the course of one of these ulcers is not straight; it is very rugged and it gives a splendid chance for the formation of coagula.

DR. HODGEN—I would like to ask Dr. Bernays whether, if

the hemorrhage had occurred above the vocal cords, it might not still have gone into the trachea.

Dr. Bernays—If the ulceration above the trachea had been in such a place as to paralyze the nerve governing the action of the laryngeal muscles then perhaps the vocal cord would have been paralyzed and the blood could have passed down. Otherwise it would not have been likely the blood could enter the trachea, because it would have been coughed up. However, it might be possible that a hemorrhage occurring in the pharynx or the epiglottis, some blood might run into the trachea, but the hemorrhage from those parts would be very small in the first place and the question can have no bearing whatever on the result.

Dr. Hodgen—It was stated as a reason for the positive knowledge that there was a communication between a vessel, at least as large as a carotid and the trachea, that if the opening had been in the larynx or pharynx the symptoms that occurred, would have been physiologically impossible.

Dr. Johnston—When blood is thrown into the trachea, does it not create an irritation such as would cause cough?

Dr. Hodgen—Certainly, when blood flows into the trachea it does cause cough. But it would not, of course, cause the irritation that some foreign bodies would; but when a person coughs once and completely empties the lung, partially he must inhale air before he can cough again, and in doing so, the blood, if he had a hemorrhage would flow down the trachea.

Dr. Bernays—I can not agree with Dr. Hodgen in this respect. I have only seen blood flowing into the trachea from the mouth, during operations where the patient was under chloform—where the vocal cords would not close properly. I have not seen blood in a healthy person run down and get into the trachea from wounds in the pharynx, and I do not think any danger from that course need be anticipated. Then in regard to my diagnosis. I do not think the doctor's points are well taken. Of course I saw that a cast, of the kind I found in this man's bronchi, could not possibly have come from a hemorrhage above the true vocal cords unless some operation had been performed, or there had been paralysis of the true vocal cords. Hemorrhage to such an extent could not come and could not pass be-

yond the vocal cords; it is very rare, unless there are pathological states that did not exist in this case.

DR. HODGEN — Allow me to state that one of the great dangers of fracture of the hyoid bone, is the possibility of rupture of the lingual artery and the flowing of blood, running into the trachea and producing suffocation.

However, Dr. Bernays' experience may not conform to the physiological facts, it is a fact still, that it is utterly impossible that a man can have breath when the upper portion of the larynx is filled with blood without carrying that blood into the lungs before he can breathe out again; and more would flow in than would flow out, because the force of gravity would aid the inspiration. A larger percentage of blood would flow in during inspiration, than could possibly flow out during expiration And the reasons that he gives that the vessel must have been large are not sufficient; with a much smaller vessel, the same thing might have occurred.

DR. BERNAYS — I think that the opinion which was given before the post-mortem, that the vessel must have been as large as the left carotid, was justified by the rapidity with which death took place. We can calculate with mathematical accuracy what amount of blood can pass from the carotid artery during a given time, and I think I was perfectly justified in saying that death must have taken place from the hemorrhage of an artery as large as the left carotid.

Now, in regard to the fact that fracture of the hyoid bone causes rupture of the lingual artery, and death from suffocation in consequence—that may be a fact substantiated by cases, but I believe, that in those cases, death occurs rather from other causes as it did in this one.

This patient did not die from suffocation; he died from hemorrhage, and when a patient's lingual artery is ruptured in such a way that the rupture within communicates freely with the pharynx, he will certainly die quicker from hemorrhage than he will from suffocation.

A man attempted suicide, and had cut himself between the hyoid bone and the maxilla. He lost a considerable amount of blood, but the wound was closed by a surgeon in the city of Berlin before he was brought to the hospital; the stitches were closely applied, and went through the mucous membrane. The

man was taken to prison, and was attended by myself and some other of the physicians in the hospital, and one of us always on guard with him. One of the men on guard with him fell asleep, and when he awoke he found his prisoner dead. The post-mortem showed the following very curious circumstances: It showed that the hemorrhage had continued in such a manner as to bulge out the mucous membrane of the pharynx inwardly over the epiglottis and the hemorrhage continuing, and the mucous membrane being loosened from its basis, it formed a *sac* full of blood which caused suffocation by occluding the entrance to the larynx. Now in this case the hemorrhage also caused death.

Dr. Hodgen — Perhaps I misunderstood the Doctor. Did he say that in the post-mortem examination he found no blood in the trachea.

Dr. Bernays — None at all.

Dr. Hodgen — Really, I cannot understand how a wound in the trachea from the arch of the aorta as large as the carotid artery should discharge blood enough to bleed a man to death, and yet at the post-mortem find none in the trachea. I do not deny the fact, but I do not see how it is possible.

Dr. Bernays—There was some blood in the smaller bronchi—in the very small bronchi—but there was none in the larger. It seems that during the struggle all was coughed out.

Dr. Lutz — Would not that blood in the smaller bronchi be the blood that killed him?

Dr. Bernays — No, I do not think it would. There was not enough.

Dr. Lutz — I did not hear a report of this case, but the discussion is interesting to me from the fact that to-day there came to my office a woman upon whom murder had been attempted by her husband, who had committed suicide himself. Upon reaching the house, immediately after the attempt, I found the man had lost quite a quantity of blood, but not enough to suppose he would die from hemorrhage. I placed his head on the table, and hanging his head low, I made artificial respiration, and I have no doubt that the pressure which I thus made, affected his cough to such an extent, that he coughed out an

enormous quantity of blood. He then immediately began to breathe again. After a while I looked at his wife, and on returning to him he still continued to cough. This man died from the quantity of blood that had flowed into the lungs. It seems to me that it does not take much blood to plug up the smaller bronchial tubes, and it is there that death is caused. Now his wife had been similarly cut, and in her case neither the carotid arteries nor the jugular veins, nor any of the larger vessels had been wounded, but there was great dyspnœa, and I induced vomiting in her case, and during the vomiting she began to cough, and a large quantity of blood, which was coagulated and presented the appearance as though it had been molded to the tubes, came out. And I have no doubt that although there was no blood in the trachea in either of these cases, both would have died from the blood which flowed into the smaller bronchial tubes.

SATURDAY, MAY 29TH, 1880.

DR. WILLIAMS—I would like very much to ask if any of the members present have had any experience with bromide of ethyl as an anæsthetic? I will state that I have recently seen it used for a brief operation, and liked its effects very much. The subject was a child, æt. 10, and it was not more than five minutes from the commencement of inhalation, before the operation was completed, the child fully restored to consciousness—fully recovered from its effects. The effect is quite rapid,—in this instance, taking about two minutes to get fully under the influence, after which the child snored profoundly for two minutes and awoke suddenly, fully restored, and leaving no tendency to stupor behind. This was a quick operation, but in tedious operations the drug would have to be given continuously. It is important to have the experience of physicians on this subject, if this agent is as good as chloroform, or if it is safer than chloroform, it is very desirable that we should all know it.

DR. W. JOHNSTON—The bromide of ethyl has been tried in a number of cases, and we have unfavorable reports from some quarters; some have found it difficult to use, and in some in-

stances requiring a resort to chloroform to complete the operation. As many as two deaths have been reported, as due to its use.

Dr. Hurt—What was the nature of the operation?

Dr. Williams—I did not say, because I did not consider that material. It was some form of staphyloma, and required a very short operation. The bromide of ethyl was given in that case exactly like we are in the hibit of giving ether; the patient was smothered down with it, just as we do with ether. It must be held down closely and the atmosphere excluded from the patient, so as to make the inhalation as concentrated as possible. At least, this was the way I saw it given.

Dr. McPheeters—I am very glad that Dr. Williams has given us a more complete report of this case, and I would not concur with him in the insignificance of the operation in relation to the anæsthetic used, because certainly the value of any particular anæsthetic will depend upon the nature of the operation, and the time required to perform it. In cases requiring only a few moments to complete the operation, this perhaps will be the proper anæsthetic. In other cases which could be enumerated it would perhaps be found inadequate, and would be a failure. That is all there is about it.

Dr. Williams—In the report that comes from Philadelphia, and also in the report that comes from Cincinnati, they have kept persons under the influence of bromide of ethyl constantly and completely for an hour, and an hour and a half, and that is certainly long enough to make any ordinary operation, and the effect of the drug can be kept up by simply continuing to inhale it.

In the case I have reported, as soon as the child went to sleep and began to snore, the drug was withdrawn. Dr.——, of Cincinnati, reports recently several cases, eight or nine I believe, where he had given bromide of ethyl, and spoke quite favorably of it in his report to the Cincinnati Medical Society, and the Philadelphia report speaks very highly of it; Dr. Levis speaks very highly of it. He has never met with any accident. He thinks it is safe and more pleasent than either of the other anæsthetics.

Dr. Post—I was not present at the experiment, but I have

been told by one who was present, that bromide of ethyl was used at the Sisters' Hospital, but through some cause the result was not satisfactory. Beyond that, I have no further knowledge of it. Several gentlemen have asked me if I have seen it used. I think it has been used a number of times here.

DR. WM. PORTER—I will say that I have given this anæsthetic three times. By repeated inhalations the effect was kept up. In the case where I gave it the patient did very well. There was a bad odor for some time, and yet it was more disagreeable to those about than to the patient. I prefer a pure article of chloroform when it is got from the proper source. I have seen it given under every condition. I never saw but one death from chloroform in my life. I have given chloroform for four hours successively. I believe where death occurs it is the result of criminal carelessness; I have seen persons administer chloroform who had no business to give a dose of calomel.

DR. MCPHEETERS—I think the day is not far distant when the medical man who uses chloroform, on account of the uncertainty of its action, will be liable to prosecution. I know that there are some medical societies, I think the Medical Society of Boston, for one, who have already, on account of the danger, passed resolutions that they are not justified in using it. Every time we use chloroform it is an experiment. If the patient has taken it a dozen times it does not follow that the thirteenth time it will not prove fatal. It has proved fatal in careful hands in the smallest amounts. There is great uncertainty about it. If we can give bromide of ethyl, or anything else in place of it, I think we ought to do it. I think it is less used now than formerly. The uncertainty of its operation is great, and the number of deaths are numerous There is an increased opposition to it on account of its uncertainty. There was a medical friend of mine who came near losing a patient not long ago by the use of a very moderate amount of chloroform in labor. The patient, as he gave me the particulars, became blue in the face and vomited blood, and the pulse became almost imperceptible. The only explanation I can give for the safety of it in labor is that it is modified by the pain. In ordinary surgery I see enough to convince me that it is uncertain and unsafe.

DR. DUDLEY—I consider it strange that the gentleman goes to

such an extreme. I do not think there is any danger at all. It has been my misfortune to see and to know of several deaths from chloroform, under the hands of the most eminent surgeons in St. Louis. I have witnessed the deaths of two persons from chloroform, and held an inquest upon the body of another, who died under its use, but in these cases that came under my observation I am not prepared at all to take the position which Dr. McPheeters takes: that those who administered this important anæsthetic should be regarded as manslaughterers.

Dr. Lemon—I would like to enter my protest against chloroform; I once had a death under my own hands. Although Dr. Porter argues that because *he* has never seen a fatal case, therefore chloroform is safe, I must differ with him, for I know that in the fatal case to which I allude, the anæsthetic was given carefully and in a small quantity, yet the patient died suddenly.

Dr. Lutz—I have invariably administered chloroform. No one doubts that there is danger connnected with it, and indeed, there is some with all anæsthetics. Proper precautions are not always used; I have heard a gentleman assert that chloroform can be used where there is diseased heart, but I would never do it. I would employ a substitute; ether is the best. In all cases you must watch your patient.

Dr. Lemon—I do not think that heart disease cuts a very great figure in the case. I have seen chloroform administered to a good many cases with heart disease. They took it with the greatest of ease. This case that I was giving it to, was, as our post-mortem examination showed, a perfectly healthy man. His heart was perfectly healthy; the lungs were healthy. He had not taken at this time more than a drachm and a half of chloroform.

Dr. Bernays—I am a great friend of chloroform, and the opposition that Dr. McPheeters makes to it is merely a personal preference. If I had the misfortune to lose a patient while giving chloroform I would not hesitate a moment to have the Doctor called as an expert in case I was brought to trial for manslaughter. He has a preference for ether because he thinks it is the best thing. When I have a chance I always give chloroform. I believe there is very little danger connected with it, although there is some, and the reason we hear of so many

deaths now-a-days is because there are more cases. I think the ratio is not changed; I do not think more than one out of 20,000 die from the use of chloroform. I have seen a person kept under the influence of chloroform four hours, consecutively. Still there is some danger connected with giving it. We cannot deny that. One point in connection with Dr. Lutz's remarks: he said in case of heart disease he would give ether. I would like him to tell me why ether is less dangerous to administer than chloroform in heart disease? I do not think ether is any less dangerous to administer under any circumstances, but particularly in heart disease. I never heard of it being mentioned as less dangerous.

DR. STEVENS—The time was only a few years ago, within the memory of most of us here, when neither chloroform nor ether were used at all, and we remember that we frequently heard of deaths from nervous shocks. I really believe that a greater number of deaths occurred from nervous shocks than have occurred from the use of chloroform or ether. I remember operating upon a boy, about 10 years old, removing a large wen, as it was called, from the shoulder. The child died from nervous shock. So that I really believe so far as there being any crime in the use of chloroform, even in a simple operation, that the physician would be more to blame for operating without than with it.

DR. PORTER—The case Dr. Lutz has reported, taken in connection with this interesting case, conveys to us, I think, a very important lesson. The case which Dr. Bernays has reported is in many respects unique in the annals of this Society. The points as I understand them, are these: The man, who had had syphilis, had a severe hemorrhage which possibly itself ceased without much treatment. The second hemorrhage occurred most severely, but after the second hemorrhage an ulcer of the trachea was diagnosed, which finally perforated the aorta. But, Mr. President, after a careful although but a brief examination of this specimen, it seems to me this was not an ulcer of the trachea. There are old ulcers in the aorta. It has been proved that this is no more favorable site for syphilitic deposits than the larger blood vessels of the thorax, and here we find in the aorta old cicatrices. We find just about the ulceration a small deposit, very likely a syphilitic deposit, and then we

find that ulcer perforating from the aorta directly into the trachea. I believe the ulcer was in the aorta. I believe the specimen will show that just beneath the mucous membrane you will find some coagulated fibrin still left. It is needless to say that if there had been syphilitic ulceration into the trachea at that point running into the aorta, that there would have been in this specimen other lesions. But the fact is that the vocal cords, the epiglottis, the tracheal mucous membrane, the laryngeal mucous membrane all seem to be entirely free except at this point. I certainly believe that the present condition of the pathological specimen warrants the theory that the initiation of the disease was in the aorta.

Dr. Johnston—How long ago is it, Dr. Hodgen, since you treated this patient for syphilis?

Dr. Hodgen—I cannot recall the case.

Dr. Bernays=As regards the remarks of Dr. Porter, I can only characterize them by saying that they are made upon a very superficial examination. As far as the primary lesion is concerned, it may have been first in the aorta or first in the trachea, but that is entirely immaterial. I believe that the breaking down was first in the lymph gland that caused adhesion between the aorta and the trachea, but afterwards the ulceration in both parts—in the trachea and the aorta—going on that the communication took place.

Dr. Mudd—The specimen is a very rare one, and it is well worth examination, and if the gentlemen would take it and look at it carefully, I think they could see that the opening in the aorta has a smoothe round even edge, it is not matter that was formed in a moment, it was not located where the rupture occurred last, it is located where the chronic lesion occured, it was the breaking down of the aorta that occurred first, and the little sac formed out side of the aorta in the trachea in which the blood was circulating, and afterwards a little aneurism was formed there and broke.

Book Reviews.

LESSONS IN GYNECOLOGY. By WILLIAM GOODELL, A. M., M. D., Professor of Clinical Gynecology in the Unversity of Pennsylvania, etc. 8 vo., pp.454, with 92 illustrations. [Philadelphia, Pa.: D. G. Brinton, 115 S. 7th St., 1880.]

If the merit of a work rests upon its popularity and the demand made for it, Prof. Goodell's present work is certainly deserving to a high degree. Within less than a twelve-month, not only has the first edition been exhausted, but a second has made its appearance greatly improved by the author. The first edition showed the book to be a valuable and practical contribution and now that it is revised and enlarged its value is so much the more enhanced. Each chapter has been remodeled, additions being made up to date. Besides this some new "Lessons" have been added together with illustrations. The great favor with which the first edition was received is perhaps the best recommendation for the second. The last three "Lessons" are such as we do not find in the majority of works on gyneecology and are of sufficient importance to demand the attention of the physician. Lesson XXXI gives "some practical hints for the prevention of uterine disorders;" lesson XXXII. "The relations which faulty closet accommodations bear to diseases of women;" and lesson XXXIII, "the sexual relations as causes of uterine disorders," including in this lesson sexual onanism and the question of "checks."

The author has a style which enlists his reader and which at the same time is practical. He is always to the point and is little given to theorizing. Typography is neat and with but few errors.

A Treaties on Foreign Bodies in Surgical Practice. By Alfred Coulet, M. D., Adjutant surgeon-major inspector of the school for military medicine at Val-de-grace. Volume I pp. 271. Volume II. pp. 420. [New York: Wm. Wood, 1880.]

This work treats of foreign bodies in general and of foreign bodies in the intestinal tract and in the pharynx and œsophagus. The author goes into detail in mentioning all the possible and almost impossible places in which bodies may be lodged. He is very accurate in giving his diagnosis by which the place of concealment of bodies may be discovered. He has certainly been successful in giving a clear exposition of the various accidents produced by the most of foreign bodies. The work must prove of interest to the general profession.

The Venereal Diseases including Stricture of male Urethra. By E. L. Keyes, A. M., M. D., Prof. of Dermatology and Adjunct Professor of Surgery in the Bellevue Hospital Med, Col .,etc., pp. 348. [New York: Wm. Wood & Co.]

This handsome octave is dedicated to Dr. Van Buren, and is designed to be one of a series addressed to the general medical practitioner. The aim has been to present the various venereal diseases as clearly as possible, avoiding such unnecessary refinement upon theoretical and mooted points as would be apt to lead to confusion or error. Dr. Keys raises his voice in protest against the new school in urethral pathology, which seems to claim that every natural undulation in the tissues of the pendulous urethra is a stricture fit for cutting, and that all the ills of the genito-urinary passages may be accounted for by the existence of these undulations and usually made to disappear when the latter are cut. He holds that the theories of the new school are as ingenious as their instruments, but unfortunately they seem to leave out of view, that the disease for which the patient seeks relief is only a symptom. The work is ably written, and will no doubt be popular with the profession.

The Surgery, Surgical Pathology and Surgical Anatomy of Female Pelvic Organs, in a series of plates, taken from nature with commentaries, Notes and Cases, by Henry Savage, M. D., London, Fellow of the Royal College of Surgeons of England, one of the Consulting Medical Officers of the Samaritan Hospital for Women. Third edition, revised and greatly extended, 32 plates and 22 wood engravings, with special illustrations of the operations vesico-vaginal fistula, ovariotomy, and perineal operations. pp. 115. [New York: William Wood & Co. 1880].

This edition has been carefully revised throughout and thirty engravings added. He has attempted a classification of neoplasms incidental to the uterine system on the basis of pathological histology of the day, omitting questions purely speculative, not going beyond what he has been able to observe and confirm. On page 125 he has give a summary of his impressions in regard to ovarian gastrotomy; they are the results of his experience founded on his prolonged coöperation with Mr. Wells as his assistant as well as colleague.

This is certainly a very valuable edition to the library and itself is worth the whole year's subscription.

Books and Pamphlets Received.

Macmillan & Co. have in press for publication in the early fall, a book which is likely to be of value to the medical profession and of advantage to the general public, entitled "Food for Invalids," and is written by Dr. J. Milner Fothergill, of London, and Dr. H. C. Wood, of Philadelphia. The aim of the authors has been to furnish the profession with a standard reference book on the subject treated.

Minutes of the Twenty-Fourth and Twenty-Fifth Annual Meeting of the State Medical Society, of Kentucky, 1879 and 1880.

Michigan College of Medicine, Corner of Antoine & Catherine Streets, and Gratiot Ave., Detroit, Mich. [Announcement for Session of 1880 and 1881].

The Detroit Medical College, Detroit, Mich. Thirteenth Annual Announcement and Catalogue, 1880–81.

How Vivisection Concerns Every Citizen, by Lewis S. Pilcher, M. D. [Reprinted from *The Christian Advocate*, of July 8, 1880.

Transactions of the American Dermatological Association with the President's Address, at the Third Annual Meeting, held at the Park Avenue Hotel, New York, August 26th, 27th and 28th, 1879. Official Reports of the Proceedings by the Secretary, Dr. R. W. Taylor.

Second Annual Announcement of the College of Physicians and Surgeons of St. Joseph, Mo.

Second Edition, May 1880. Announcement of the Medical Department of the University of Pensylvania for the One Hundred and Fifteenth Annual Session.

Diabetic Cataract, Iritis, etc. A Clinical Lecture Delivered at the Michigan College of Medicine. By C. J. Lundy, M. D., Reprinted from Michigan *Med. News*, June 10, 1880.

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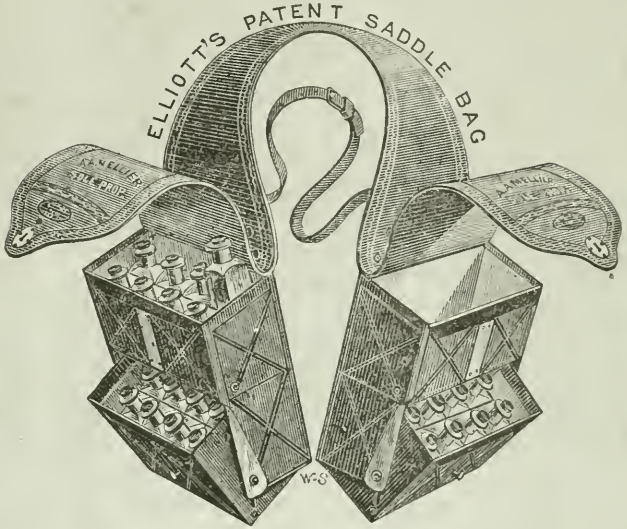

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